PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXXIII.

PART I.

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ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions,* take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions,* that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion,

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as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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ADJUDICATION of the MEDALS of the ROYAL SOCIETY by His ROYAL HIGHNESS the PRESIDENT and COUNCIL.

A Copley Medal, for the year 1832, to Michael Faraday, Esq. D.C.L. F.R.S. Fullerian Professor of Chemistry in the Royal Institution of Great Britain, for his Discovery of Magneto-Electricity, as developed by him in his "Experimental Researches in Electricity," first and second series, published in the Philosophical Transactions for the Year 1832.

A Copley Medal, for the year 1832, to the Baron Simeon Denis Poisson, of Paris, Foreign Member of the Royal Society, for his Work, entitled, "Nouvelle Théorie de l'Action Capillaire."

The Rumford Medal, for the biennial period ending in the year 1832, to John Frederic Daniell, Esq. F.R.S. Professor of Chemistry in King's College, London, for his paper, entitled, "Further Experiments with a new Register Pyrometer for measuring the Expansion of Solids," published in the Philosophical Transactions for the year 1831.

The Royal Medal, in the department of Astronomy, for the year 1833, to Sir John Frederick William Herschel, K.H. F.R.S. &c. for his paper, "On the Investigation of the Orbits of Revolving Double Stars," inserted in the fifth volume of the Memoirs of the Royal Astronomical Society of London.

The Royal Medal in the department of Physiology, for the year 1833, to Professor Augustin Pyramus De Candolle, of Geneva, Foreign Member of the Royal Society, for his discoveries and investigations in vegetable physiology, as detailed in his Work, entitled, "Physiologie Végétale," and published in the year 1832.

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PHILOSOPHICAL TRANSACTIONS.

I. *An Account of the Construction of a Fluid Lens Refracting Telescope of eight inches aperture, and eight feet and three quarters in length, made for the Royal Society by* GEORGE DOLLOND, *Esq. F.R.S. By* PETER BARLOW, *Esq. F.R.S. F.R.A.S. M.C.P.S. Cor. Mem. Inst. France, &c.*

Read November 22, 1832.

IN my former papers on the construction and performance of fluid refracting telescopes with open lenses, I have pointed out the great variety of cases included in the general formula, which have since been increased by the ingenious construction of Mr. ROGERS, and have referred to the difficulty of carrying one's mind through all their intricacy, so as to select, independently of experiment, from amongst the several cases, that which would produce the best result. The form I gave to my original construction was founded principally upon the idea of lengthening the focus beyond the length of the tube, and as far as that object was concerned, the result was perfectly satisfactory; but, as I have stated in my description of that instrument, it was found, as this principle was extended, that the perfect part of the field became more contracted, so as to render it questionable at what point to stop, to produce upon the whole the best effect. Other considerations also presented themselves, and I wished therefore to have the means of making certain preliminary experiments, with a view to the determination of a few such practical points, before selecting out of the multiplicity of arrangements (all theoretically true,) that which should be adopted in the construction of so large a telescope as that which I ventured to propose in my last paper, Phil. Trans. for 1831. The advantage, if not the absolute necessity, of such experiments in a novel case of this kind must be obvious to every one who has had any experience in practical optics, where having several objects to fulfil, interfering more or less with each other, it is only by experiment it can be ascertained how far an advantage can be pushed on one side without making too great a sacrifice on another; and such experiments are too expensive to be conveniently carried on by a private individual who has no other object in their success than the advancement of optical and astronomical science.

The Council of the Royal Society therefore appointed a committee to report upon my former paper, who were of opinion that it would be advisable to proceed at once to the construction of an eight-inch telescope for the Royal Society, the success of which might decide the question of proceeding upon the scale I had proposed. They accordingly ordered a telescope of the above dimensions of Mr. Dollond, leaving to me the arrangements relative to curvatures, focal length, &c. After due consideration, I determined on the adoption of a form and principle of construction which I thought likely to embrace the most advantages, and I am in hopes the result will not be considered unsatisfactory, although I have in the progress of the construction seen one or two changes which, in a future case, might be advantageously adopted.

The aperture of the telescope is eight inches clear glass, and focal length eight feet and three quarters. The spherical aberration is perfectly corrected, the field is open and flat, with abundance of light,—all very desirable qualities in an astronomical telescope; with respect to colour, there is perhaps more outstanding on the violet side of the spectrum, than is generally found in the usual refractor, particularly towards the limits of the field. I estimate the extent of the field by the product of the minutes it contains, multiplied by the power, and I find this product with my four powers, varying from 100 to 450, ranges between 3000 and 3450, which rather exceeds perhaps the usual fields of refractors; at all events there is a great improvement in this respect as compared with my former instrument.

As to the length, although I am unwilling to think that upon the whole the telescope is too short, yet it must be admitted that where minute definition is wanted, an advantage is gained by contracting the aperture, but the whole is certainly available for bringing to light very small stars; and probably more distinctness with the whole aperture might be derived from a little increase in the aperture of the fluid lens.

Having made these few general remarks, I shall endeavour to explain the reasons which induced me to select the particular case I have done, and the principles on which I have established the equations of condition. In my former paper I have explained my reason for using a double instead of a single front lens. It moreover occurred to me, that although the chromatic dispersion of a lens of any medium, or the coloured part of its focus, bears a fixed ratio to its mean focal length, this is not the case at the first immersion of a ray and during its progress in the medium itself, a remark I do not remember to have seen made by any optical writer. Also, as the dispersion of a lens depends upon the relative indices of the extreme and mean rays of the spectrum, this will vary according to the medium from which a lens receives and into which it transmits those rays, so that the dispersion of the fluid will be much greater receiving and transmitting rays into glass than into air.

To take advantage of this circumstance I resolved to make all the correction for colour and aberration in the passage of the rays from one fluid shell to the other, by causing the mean rays from the front compound lens to impinge perpendicularly on the first surface of the fluid shell, and again perpendicularly on the fourth surface, and thereby, of course, preventing any further chromatic development.

By this arrangement, the dispersion or coloured part of the focus of the front compound lens is reduced on entering the first shell of the fluid lens, in the ratio of about 3 to 2, while that of the fluid is so much increased, that the dispersive ratio of the two during the passage of the ray through them is reduced to ·08, whereas, in the usual refractor, it is about ·60 or ·65; the dispersive ratio is therefore only about one eighth of its usual amount, and the spherical aberration is also reduced by the nature of the construction in about the same proportion, that is, to about one third, in consequence of the double front lens, and to about one third again by the focus being nearly three times as long as that of the crown lens of the usual refractor.

There was only one doubtful point connected with this form, which was, that as in providing for the correction of the spherical aberration we can only effect this for one index (generally the mean), the aberration of the extreme rays must remain imperfectly corrected, which imperfection is greater as the indices of the extreme rays bear a higher ratio to the mean index; and thus it constantly happens in constructions of this kind, that while we are advancing towards perfection on one hand, a corresponding evil enters on the other, a circumstance which renders preliminary experiments so highly necessary in everything connected with practical optics. As however the whole linear amount of the aberration of the front lens does not amount to $\frac{7}{100}$ths of an inch, I fully determined upon adopting this principle of construction, as it appeared to me that, with so little to correct, the imperfection must necessarily be very inconsiderable. This circumstance, however, led me to consider what might be the advantages of making the correction for the spherical aberration of the front lens, by means of the glass shells of the fluid lens, reducing that of the fluid either to zero or to a minimum. And on making independent calculations on both hypotheses, I found, what was perfectly unexpected to me, that the same fluid lens would answer both cases by simply inverting it, or so nearly so, at least, as not to render it necessary to have a second fluid lens made to decide the question.

With these decided advantages, according to either principle, it was obvious that I might venture upon a much shorter telescope than would be required for an eight-inch aperture of the usual kind of refractor; but the want of preliminary experiments made it impossible to know to what extent this shortening might be carried. I determined therefore on eight-feet focus; but the nearest I could come to this length without having new tools made, was eight feet nine inches.

Another principle, which was thought desirable, was to divide the refraction pretty equally between all the four surfaces of the two front lenses; and this condition was quite consistent with the adoption of two equal convex lenses, as will be seen immediately by taking ·500 as an approximate index of crown glass, and ·100 inches as the compound focus; for then the first refraction will make the rays converge to 300 inches, the second refraction to 200 inches, the third to 150 inches, and the fourth to 100 inches. If however I had to repeat the experiment, I should prefer making all the refractions at the first three surfaces, by causing the light to fall perpendicularly on the last, as in that case the passage of the rays, after entering the second lens, to the focus, would (employing the first of the above principles,) be made in precisely the same way as in an uniform cylinder of glass, as in Mr. CODDINGTON's microscope; with the exception of the slight refraction they would sustain in passing through the fluid, which must necessarily be very inconsiderable, as its negative focal length is more than six hundred inches. Moreover, besides this advantage, which may be perhaps rather apparent than real, this form would have given less spherical aberration, and consequently the curvature of the fluid might have been still further reduced, which is at all events a real advantage, but it did not occur to me till I had proceeded too far to make any alteration.

It will be seen from what has now been stated, that the conditions I proposed to myself were:

1st, That the front lens should be composed of two plano-convex lenses of equal focal length.

2ndly, That the curvatures of the fluid shells should be such that the mean rays, after leaving the front lens, should fall perpendicularly on the first surface.

3rdly, That the corrections for colour and aberration should be effected in the passage of the rays through the fluid.

4thly, That they should then impinge perpendicularly on the fourth surface, and be thence transmitted aplanatic to the focus.

Now it may be observed, that as far as relates to establishing the equations of condition, these will be the same as if the rays were refracted at each surface of the fluid lens from, and into, air; and as this method of considering the subject will throw some facilities into the investigation, it is adopted in what follows; but it is impossible not to believe that the practical effect must be very different in the two cases, seeing that in one the ray would have to sustain six considerable refractions, correcting each other by their positive and negative effects, while in the other there are only two inconsiderable refractions, equal to the difference.

Having thus settled the general principle of construction, the next step was to determine the dispersive ratio between the fluid and crown glass, these two media having never before to my knowledge been brought into optical combination. This was done by applying a crown lens to the dispersive instrument, described in the Phil. Trans. for 1827, p. 235; and as a proof of the accuracy of that method, it may be stated, that the calculations founded on the numbers thus obtained, answered most accurately to the focal length intended, and that the corrections for colour and spherical aberration were both as nearly perfect as is perhaps ever to be expected.

The numbers found as above, were

Index of sulphuret of carbon . . . $1+a'=1·6343$
——— crown glass . . . . $1+a=1·5396$
Dispersive ratio 1 : 3·333.

With these data and conditions we must now proceed to determine the radii of curvature of the lenses, by establishing and solving the following equations.

Let $nf=$ the focal length of the compound front lens.

$f=$ the part of that focus which falls beyond the fluid lens; and consequently, according to our condition,

$f=$ also the radius of curvature of the front surface of the first fluid shell.

$\frac{1}{x}=$ the sum of the reciprocals of the radii of curvature of the interior surfaces of the two shells, and consequently also of the fluid.

$y=$ the distance of the back surface of the second fluid shell from the focus; and consequently also, by our condition,

$y=$ the radius of the concave curvature of the back surface.

$1+a$ the index of crown glass.

$1+a'$ the index of the fluid; and

$a'-a=a''$.

Then it is obvious we must have

$$\frac{1}{f}+\left(\frac{1}{f}+\frac{1}{x}\right)a-\frac{1}{x}a'-\frac{1}{y}a=\frac{1}{y}$$

or

$$\frac{1+a}{f}-\frac{1+a}{y}=\frac{a''}{x}$$

whence

$$x=\frac{a''fy}{(a+1)(y-f)}\quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (1)$$

This equation has reference only to focus. We must now provide for achromatism, and for this purpose must determine the dispersive power of all the crown lens, considered as a simple lens, situate in the place of the fluid.

To effect this,

Let δ = the absolute dispersive power of crown glass.

$n\delta$ = the dispersive power of the front compound lens, as referred to its remaining focus at the place of the fluid lens.

f° = the focal length of the shells of the fluid lens.

Then $$\left(\frac{1}{f}+\frac{1}{x}-\frac{1}{y}\right)a=\frac{1}{f^\circ}$$

or $$f^\circ=\frac{fxy}{axy+af(y-x)} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2)$$

Now the dispersive power of these combined lenses, with that of the front lenses, reckoned from the place of the fluid, will be (denoting that power by δ'')

$$\delta''=\frac{f\delta+nf^\circ\delta}{f+f^\circ}$$

(See Journal of the Royal Institution, No. IV. p. 6.)

And to produce achromatism (remembering that $\frac{x}{a'}$ = focal length of the fluid, and $\frac{ff^\circ}{f+f^\circ}$ = the combined length of all the crown lenses,) we must have (calling δ''' the dispersive power of the fluid), to produce achromatism,

$$\frac{ff^\circ}{f+f^\circ}:\frac{x}{a'}::\delta'':\delta'''$$

Or substituting for δ'' its value as above found, and writing $m\delta$ for δ''', (m being the dispersive ratio between crown glass and the fluid,) we have

$$\frac{ff^\circ}{f+f^\circ}:\frac{x}{a}::\frac{f\delta+nf^\circ\delta}{f+f^\circ}:m\delta,$$

which when reduced gives

$$f^\circ=\frac{fx}{ma'f-nx}, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (3)$$

from which three equations, the values of x and y may be determined. That is, we first find

$$y=\frac{(ma'-a)(1+a)f-aa''f}{(ma'-a)(1+a)-(a+n)a''}$$

and then x is readily determined by means of equations (1) and (2).

In this expression for y; m, a, a', a'' are given quantities, but n is assumable at pleasure, within all practicable limits. In my former construction I found the rationality of the spectrum best preserved when n was taken $= 2$, and taking it so in the present instance with the proper value of the other quantities, viz. $a = \cdot540$, $a' = \cdot634$, $a'' = \cdot094$, and $m = 3\cdot333$, we have

$$y = \frac{1\cdot573 \times 1\cdot540 - \cdot05076}{1\cdot573 \times 1\cdot540 - \cdot23876} f = 1\cdot086 f,$$

consequently $2\cdot086 f = l =$ the whole length.

Hence any length whatever being assumed for a proposed telescope, or any value of f, all the other quantities may be immediately found. It is also quite indifferent what length is assumed, as all the other quantities will be proportional.

Assuming then a length $l = 150$ inches, we have

$$f = \frac{150}{2\cdot086} = 71\cdot9 \text{ inches}$$

$$y = 1\cdot086 f = 78\cdot1 \text{ inches, and}$$

$nf = 143\cdot8$ inches, the combined focus of the front lens, and equation (1) $x = 55\cdot3$ inches, or

$\frac{1}{x} = \frac{1}{55\cdot3} =$ the sum of the reciprocals of the radii of the interior surfaces of the fluid shells, or of the fluid itself, considered as negative.

If therefore in the particular case in question we denote the radii of the several curvatures of the fluid shells by r, r', r'', r''', we shall have $r = 71\cdot9$, $r''' = -78\cdot1$, and

$$\frac{1}{r'} + \frac{1}{r''} = \frac{1}{55\cdot3}$$

and the whole focal length of the two combined front lenses $= 143\cdot8$ inches.

Thus far then we have provided for focal length and achromatism, and have still r' and r'' undetermined, but which must now be found from these conditions; viz. so that

$$\frac{1}{r'} + \frac{1}{r''} = \frac{1}{55\cdot3}$$

and that the spherical aberration at these surfaces may correct that of the front lenses, the radii of whose surfaces might also be left undetermined, we having at present only considered their focal length. It is proposed, however, that in this case these lenses shall be both equal plano-convex, which limits their radii, these being found one hundred and fifty-five inches.

The formula I employ for this determination is that given in Phil. Trans. for 1827, p. 247, observing only, that what is there denoted by d, which is the dispersive ratio, or the ratio of the focal length of the two lenses, must here be modified, in consequence of the lenses not being in contact, and the aberration being produced in the passage of the rays through the fluid, from glass to glass, that is, the effective index is on this account found by the proportion

$$1{\cdot}540 : 1{\cdot}634 :: 1 : 1{\cdot}061$$

we must therefore consider $a' = {\cdot}061$

and the effective focal length of the fluid $= -\frac{x}{a'} = -906{\cdot}6$ inches $= f'$.

Referring now to the original formula above quoted, and putting C to denote the first factor, we shall find that the real equation is

$$\mathrm{C} \times \frac{a' y'^2}{2f' a' (q' + 1)} = \frac{p y^2}{2fa}$$

which, in the common form, as $y' = y$, and $\frac{f}{f'} = d$, becomes

$$\mathrm{C} \times \frac{d a}{q' + 1} = p$$

But in our case $y' = \frac{1}{2} y$; and our formula therefore is

$$\mathrm{C} \times \frac{fa}{4f'(q' + 1)}$$

consequent $d = \frac{fa}{4f'} = {\cdot}0398$.

In the calculation for correcting aberration, we must therefore take p and d as above, $a = {\cdot}540$, $a' = {\cdot}061$, and then proceed exactly as in common cases, except that in this instance, as it is proposed to form the front lens of two plano-convex lenses, we must find the amount of aberration for such a combination, and then determine the ratio $r' : r''$ that shall produce in the fluid such amount of aberration as may correct that of the front lens; whereas, in the usual case, we first find the aberration of the concave lens, and adjust the front lens accordingly. By the latter method there is only required the solution of a quadratic equation, whereas, in reversing the operation, as here proposed, we arrive at a very intricate equation of difficult solution. Instead therefore of attempting a direct process by leaving the ratio $\frac{r'}{r''} = q$ indeterminate, it will be best to proceed by the method of position, which, though less elegant, is much more simple and expeditious. First, then, let us compute the amount of spherical aberration for parallel rays produced by two plano-convex lenses, the convex side of each being turned towards the object.

The aberration of a single convex lens, convex in front, whose focus is $f_{\prime}$ and diameter $2\,y$, is

$$\text{aberration} = \frac{a^4 + 2\,a^3 + 1}{2\,a\,(a^3 + 2\,a^2 + a)} \times \frac{y^2}{f_{\prime}}$$

which, when $a = {\cdot}540$, becomes $\frac{1{\cdot}012\,y^2}{f_{\prime}}$

and since the focus of the combined lenses is 143·8 inches, that of this single lens is $f_{\prime} = 287{\cdot}8$ inches, and $\frac{287{\cdot}8}{{\cdot}540} = 155$ inches = radius of the front surface.

At the second lens, therefore, the rays are converging to a distance $d = -\,287{\cdot}6$; and this lens being equal to the former, its radius $r = 155{\cdot}3$, r' being infinite, and it remains to determine its aberration.

Calling $\frac{d}{r} = \frac{-\,287{\cdot}6}{155{\cdot}3} = m = -\,1{\cdot}85$, and

$$\frac{(a + 1)\,d\,r}{a\,d - r} = d' = 1{\cdot}424\,r$$

$$\frac{a}{a + 1} = b = {\cdot}35$$

the formula for the aberration (Phil. Trans. for 1827, p. 243,) becomes

$$\left.\begin{array}{l} \frac{a\,(m + 1)^2}{(a\,m - 1)^2} \times \frac{m + a + 2}{(a + 1)\,m} \times \frac{1 - b}{2\,a} \\ + \frac{b}{1} \times \frac{2 - b}{2\,(b - 1) \times 1{\cdot}424\,r} \end{array}\right\} \times \frac{y^2}{f_{\prime}}$$

which in numbers gives

$$\text{aberration} = \frac{{\cdot}5629\,y^2}{f_{\prime}}$$

of the second lens only. We have already found in the first lens, aberration

$= \frac{1{\cdot}012\,y^2}{f_1}$; but this is reduced by the second lens to one fourth its amount, or to $\frac{253\,y^2}{f_1}$, whence the aberration of the combined lenses is $\frac{{\cdot}8159\,y^2}{f_1}$ or $\frac{{\cdot}4079\,y^2}{nf}$, nf denoting the compound focus $= \frac{1}{2} f_1$ according to our first notation.

And it remains to find such a ratio of $r' : r''$ or such a value of $\frac{r''}{r'} = q'$ as shall produce the aberration in question.

The formulæ for the calculation of this aberration with any given value of q' already referred to in the Phil. Trans. are,

$$^*c = \frac{f''}{r'} \qquad c' = \frac{(a'+1)f''q'}{a'f''-r''} \qquad b = \frac{a}{a+1}, \text{ and then}$$

$$\left.\begin{matrix} \frac{(c+q')^2}{(a'c-q')^2} \times \frac{c+(a'+2)q'}{c(a'c'+a'+1)^2} \\ + \frac{(c'+1)^2}{(bc'+1)^2} \times \frac{(c'+2-b)q'}{c'} \end{matrix}\right\} \frac{a\,d}{q'+1} = p$$

in this equation $p = {\cdot}4079$, $a = {\cdot}540$, $a' = {\cdot}061$, $f'' = y = 78$, and $d = {\cdot}0398$, $\frac{r''}{r'} = q'$ being at present undetermined, but which is now to be assumed so as to give an approximate value to p.

A few simple trials will show that the value of q' must be about -3. Assuming then $q' = -3$, we have

$$r'' = f'a'(q'+1) = 110{\cdot}6$$

$$r' = f'a'\frac{(q'+1)}{q'} = -37$$

and as we already know $r = 72$, and $r''' = -78$ (taking only their nearest integral values,) we find by substitution in the preceding formula, $p = {\cdot}4211$ instead of $p = {\cdot}4079$.

Take therefore $q = -3\frac{1}{3}$, then

$$r'' = 129, \text{ and } r' = -38{\cdot}7$$

which, by substitution, gives $p = {\cdot}3930$.

It is clear therefore that the value of q lies between the limits -3 and $-3{\cdot}333$,

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* It will be observed that r' is here the same as r'' in the original formula, and r'' the same as r'''.

and it will be observed that these values of p, vary principally as the factor $\frac{q'}{q'+1}$ varies; and treating the subject upon the common arithmetical principle of position, we find $\frac{q'}{q'+1} = 1{\cdot}4565$, or $q' = -3{\cdot}2$ very nearly.

This value of q gives

$$r'' = f'\,d'\,(q'+1) = 121{\cdot}5 \text{ inches}$$

$$r' = f'\,d'\,\frac{(q'+1)}{q'} = -38 \text{ inches.}$$

We have now therefore all the numbers answering to a total length of 150 inches, viz.

l = 150 inches whole length,
r = 72 inches first surface of fluid shell,
r' = 38 inches second surface,
r'' = $-$ 121·5 = third surface,
r''' = $-$ 78 inches fourth surface,
R = 155·3 inches convex surface of front lens,
Δ = 72 inches distance of the lenses.

And it is only necessary for any proposed lengths to use these proportions.

On consulting with Mr. DOLLOND, I found that to have confined myself to eight feet length, two or three new tools would have been necessary, but by taking $l = 105$ inches, he had only occasion for one. I therefore adopted that length; for although Mr. DOLLOND was very desirous of working out my numbers to any accuracy I wished, I had no desire to urge this point to an unnecessary degree of refinement, particularly as the nature of the construction always admits of producing chromatic correction to any degree of precision.

I have already stated that the lenses worked according to these proportions, when placed in their respective cells, agreed in every respect with the computed results, as well in focus as in chromatic and spherical correction.

These remarks, it will be observed, apply to the arrangement of the fluid lens according to the first principle, in which the corrections are all made in the passage of the light through the fluid, and by the fluid only. By reversing this lens we have a telescope on the second principle, in which the correcting power of the fluid for spherical aberration is a minimum, or at least very small in comparison with that of the glass in which it is inclosed; and on trial it was found, I must admit contrary to my expectation, that the performance of the telescope was better with the lens in this position than in the former; the difference however is but little, and this little has reference only to figure; I therefore am rather inclined to attribute this preference to some accidental better adaptation of the glasses to each other when in this position than to any actual theoretical advantage. In both cases the spherical aberration is equally well corrected, there being no perceptible change of focus, whether we employ the whole aperture or the central four inches, or a simple ring of an inch of light round the margin of the lens. In each of these three cases I have repeatedly seen the small star in Rigel without the slightest change of focus. For the rest it is not my intention to offer any remarks on the performance of the telescope, as I conceive this will be best reported upon by the committee, after they have submitted it to such tests as may be judged most proper for determining its defining and penetrating power.

It only therefore remains for me to state, that I feel much indebted to Mr. DOLLOND for the readiness with which he has complied with all my suggestions, and for the accuracy with which he has executed every part of the instrument.

P.S. Since this paper was written, Mr. DOLLOND has reworked one of the surfaces, and there is now no question that the preference in the performance is decidedly with the fluid lens in its first position.

II. *On the extensive Atmosphere of Mars. In a Letter to His Royal Highness the* DUKE OF SUSSEX, *K.G., President of the Royal Society. By Sir* JAMES SOUTH, *F.R.S.*

Read December 13, 1832.

THROUGH the kindness of your ROYAL HIGHNESS I had some time since the honour of calling the attention of astronomers to the "Extensive Atmosphere of Mars"—to the observations of those great men, from which its existence was inferred,—and I showed that they were either unsupported by, or were at variance with my own. Still, however, as the observations, of which mine seemed subversive, were bequeathed us by astronomers, to whom astronomy owes deep and lasting obligations, respect due to their memory demanded that I should rather enforce the necessity of further observations than treat the matter as actually decided.

This night has put me in possession of fresh evidence, and I lose no time in forwarding it to YOUR ROYAL HIGHNESS, in the hope that it may have the honour (should you, as President of the Royal Society, think it worthy,) of being presented by you for insertion in those journals, which contain so rich a mine of astronomical truths.

During twilight, in the field of the large Equatorial*, I saw a star of the

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* This instrument, the design and work of Messrs. TROUGHTON and SIMMS, was executed by them in preference to the one hinted at by me, in a note appended to page 4 of the Phil. Trans. for 1826. It was erected in my observatory in the early part of the last year, and I fondly hoped, that long ere now I should have had the gratification of presenting an account of it to the Royal Society.

To my inexpressible grief, however, owing to its unfitness for the purposes for which it was designed, not only have fourteen months of a life advanced beyond its prime, been of necessity employed otherwise than in prosecuting those inquiries for which alone such an instrument was wanted; but at the present moment, as to when it will be fit for use, I have not data for offering even a conjecture.

In the mean time, fearing lest it should ultimately prove a total failure, during the last autumn I visited the Imperial Observatory of Dorpat, and I feel it due to the memory of the late Mr. FRAUNHOFER to hand down an apology through the same channel which conveyed the insinuation contained in the note before alluded to; inasmuch as a fortnight's residence under the roof of our celebrated eighth or seventh magnitude, south preceding the planet Mars; its place (approximately taken with the five-feet Equatorial) was right ascension, $3^h 29^m 19^s$, and its northern declination about 20° 22′; as additional identification, it was found to precede a star of the seventh or sixth magnitude, about five minutes twenty-six seconds and five tenths, and was about one minute and forty-eight seconds of a degree south of it. These preliminaries settled, the night became cloudy; but at about three hours and a half sidereal time, the sky being clear, on directing the instrument to Mars, the planet was seen somewhat more than half a minute from the star. The star, as previously noticed, was of a light blue colour, afforded a pleasing contrast with that of the planet, and was tolerably steady; the planet's limb extremely unsteady. The object-glass of the large Equatorial is 11·85 inches in its clear aperture, and has nearly nineteen feet focus. It has two finders, the one a telescope of 4·40 inches aperture, and five feet focal length; the other 2·75 inches diameter, and forty-two inches focus. They are placed parallel with the tube of the large object-glass, so that the same sidereal object presents itself in the centre of the fields of the three telescopes, at the same moment. The object-glasses of all the three are very perfect; and taking them in the order of their diameters, beginning with the largest, they were supplied with powers of 520, 250, and 120; and that the observations might not be vitiated by the unsteadiness of the polar axis, cylinders of wood were placed east and west of the large telescope, so that toward their upper extremities, they rested against that telescope's tube, whilst their lower ends were on the observatory floor. The five-feet Equatorial also was placed upon the planet with a power of 133 only. With this instrument the star was seen by one observer, till the planet's limb had reached it; with the large finder of the large Equatorial, another observer saw it, till a small segment of its disk had been cut off by the planet's limb; whilst with the small one it was distinguishable when not more than three seconds of a de-

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Associate Struve, has not only demonstrated that Fraunhofer must ever be regarded as an optician of the highest order, but that he merits our profound respect as a most powerful mechanician.

In short, such is my admiration of, I had almost said veneration for, the Dorpat Equatorial, I shall to the latest hour of my life regret, that the conditions on which the great man who made it, would have provided me with one still more powerful, I was, as stated in the note in question, imprudent enough to reject.

Observatory, Kensington, March 26, 1833.

gree distant from it. But although the planet's approach to the star was observed with the several telescopes, it is to the large Equatorial only that we must refer, with the reasonable hope of detecting any minute optical change which the star might apparently undergo.

No such phenomenon however occurred; for the star retained its light blue colour and comparative steadiness till the very instant of its occultation, which took place at $4^h\ 32^m\ 24^s\cdot7$; nor did its splendour suffer any diminution prior to disappearance, except what may fairly be attributed to the light of the planet. I saw not the slightest projection of the star upon the planet's disk.

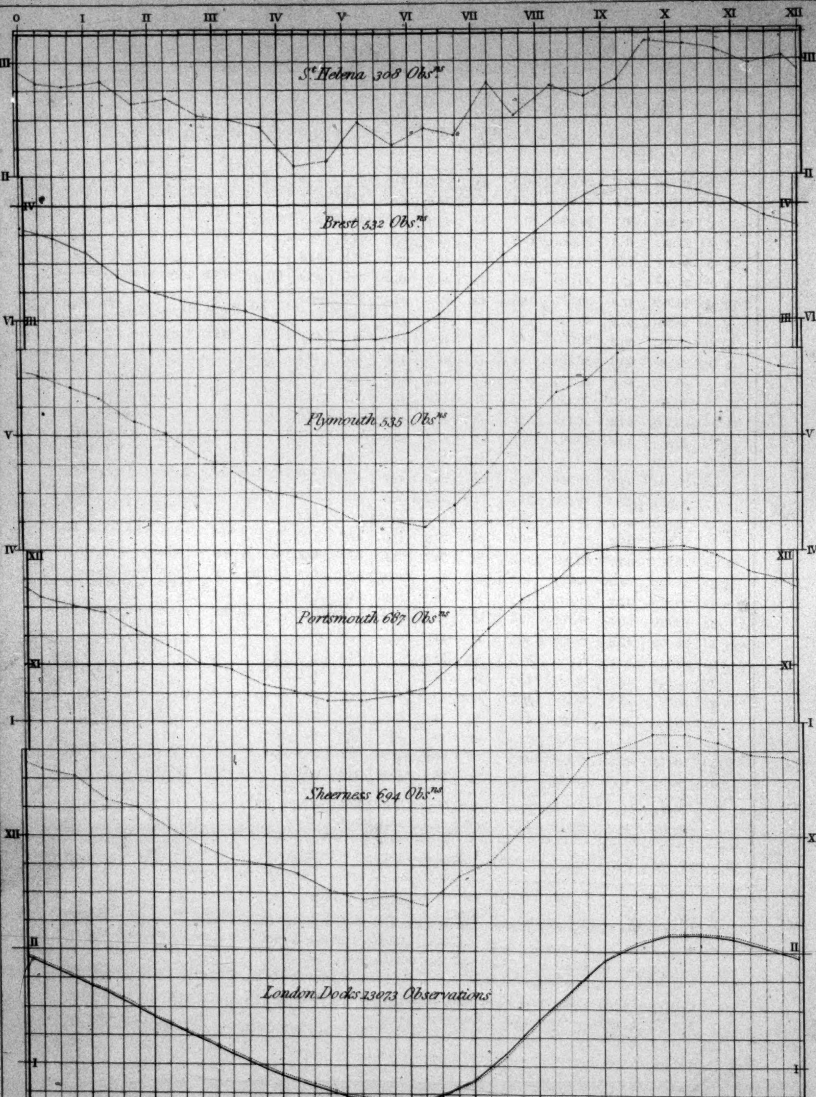
Again, at the emersion, which happened at $4^h\ 50^m\ 41^s\cdot7$, the star with the large equatorial telescope was seen neatly dichotomised; with the large finder it was detected before the planet's disk had separated from it; by the five feet Equatorial, when it was still clinging to it; and by the small finder when it was not more than three seconds distant. With the large telescope it was watched with the greatest attention, at, and for some time subsequent to the emersion; and I feel confident that not anything remarkable occurred, any more than at and previously to the immersion. The planet had passed his opposition nine days.

Reflecting on these facts and on those I have before presented to your Royal Highness, I can arrive at no other conclusion than that either some physical change has occurred in the "Extensive Atmosphere of Mars," or that the accuracy of the observations of Cassini and of Roëmer must be regarded as untenable.

Observatory, Kensington,
Nov. 29, 1832.

Semi-menstrual inequality of the Time of High Water

Phil. Trans. MDCCCXXXIII. *Plate* 2. *P.* 19.



J. & C. Walker Sculpᵗ.

In these curves, the abscissa represents the time of the Moon's transit, and the ordinate the corresponding interval between the transit & the time of High Water

Observation

Theory

III. *Note on the Tides.* *By* J. W. Lubbock, *Esq., V. P. and Treas. R.S.*

Read March 7, 1838.

By the permission of Mr. Dessiou, I am enabled to communicate to the Society some results which he has obtained, from observations made at Plymouth, Portsmouth and Sheerness, under the superintendance of the masters attendant at those dock-yards. The *establishments* of these ports (that is, the time of high water when the moon passes the meridian at 12 o'clock,) may now be considered as accurately determined. The retard $(\lambda - \lambda_{\prime})$ at Portsmouth appears to be intermediate between that at Brest and at London, being about $1^{h}\ 30^{m}$; that at Plymouth appears to be greater, and not less than that which obtains at London, for which circumstance it is difficult to account. The retard at Sheerness agrees with that observed at the London Docks. The constant which involves the mass of the moon does not differ much, as inferred from these observations at various places; it is however impossible to obtain more than a rough approximation, by these means, to that important element in astronomy, the mass of the Moon. Even a minute (of time) in the difference of the interval between the moon's transit and the corresponding time of high water, materially affects the value of the moon's mass; and insurmountable difficulties appear to be in the way of any nice determination of that quantity by these means, even if there were none of an analytical character. It would be well, by a discussion of the inevitable errors of the various data employed, to ascertain the limits of the errors which may be incurred in determining the mass of the moon by various methods, particularly through the constant of the moon's parallax.

Mr. Dessiou, with undaunted perseverance, has just completed the discussion of about 6000 more observations of the tides at the London Docks, with a view to rest upon a sure basis the corrections for the moon's parallax and declination; but these cannot be published unless he is fortunate in meeting with more encouragement than he has hitherto experienced. These results do not differ materially from those already published in the Phil. Trans. 1831, Part II. p. 413, the accuracy of which therefore they serve to confirm.

The results which Mr. **Dessiou** has obtained are not reconcileable with the theory of **Bernoulli**; therefore the Tables founded on this theory which profess to give the corrections for the moon's parallax and declination, and which are to be found in various works on navigation, but had never previously been compared with observation, must be rejected as inaccurate and utterly inapplicable.

The following Table has been formed by taking the mean of all the times of the moon's transit occurring within a given half hour, and the mean of all the corresponding times of high water according to the method explained, Phil. Trans. 1831, Part II. p. 384; the difference is of course the interval between the time of the moon's transit and the corresponding high water, which interval is given in the Table.

Plymouth.			Portsmouth.			Sheerness.		
Moon's Transit.	Corresponding Interval.	Number of Obs.	Moon's Transit.	Corresponding Interval.	Number of Obs.	Moon's Transit.	Corresponding Interval.	Number of Obs.
h m	h m		h m	h m		h m	h m	
0 14·6	5 30·9	26	0 14·6	11 35·4	29	0 14·3	+0 35·7	28
0 44	5 25·4	23	0 45·7	11 31	28	0 45	+ 32·4	31
1 10·5	5 19·5	21	1 15·6	11 24·4	28	1 15	+ 19	28
1 44·8	5 7·3	23	1 44·8	11 17·6	31	1 45·2	+ 15·1	31
2 15	5 1	22	2 15·5	11 10·2	29	2 15·5	+ 4	27
2 45·8	4 48·7	25	2 45·6	11 0·8	30	2 45	− 5	31
3 17·6	4 41·2	21	3 16·6	10 54·2	27	3 16·7	− 12·7	28
3 45·2	4 32	22	3 45·5	10 49·3	30	3 44	− 15	30
4 15·7	4 23·1	23	4 12·7	10 46·3	27	4 14·5	− 25·5	28
4 44·8	4 22·5	22	4 44·2	10 41·3	30	4 44	− 28	30
5 15·8	4 14·5	25	5 15·4	10 41·4	32	5 15·6	− 33·6	31
5 46·1	4 14·7	23	5 46·5	10 43·7	29	5 46·2	− 32·2	27
6 16·5	4 11·9	22	6 15·5	10 47·6	29	6 15·6	− 37·6	30
6 44	4 23·3	20	6 45	11 1·2	29	6 44·6	− 26·2	28
7 15	4 40·7	24	7 15·2	11 18·5	29	7 14·6	− 13·6	31
7 47·6	5 3·7	23	7 47·1	11 34·1	29	7 46·6	+ 3·4	30
8 18·6	5 22·4	21	8 18	11 44·8	29	8 17·4	+ 19·2	27
8 46·7	5 28·8	20	8 46·9	11 57·9	26	8 46·3	+ 42	27
9 16·3	5 42·7	20	9 15·2	12 1·9	28	9 15·5	+ 47	29
9 45·6	5 48·6	24	9 46·2	12 0·6	30	9 46·4	+ 53·2	29
10 16·6	5 47·5	21	10 16·7	12 1·8	28	10 16·6	+ 53·7	27
10 45·6	5 43·1	23	10 46	11 54·4	27	10 45	+ 49·8	28
11 15·2	5 41·4	20	11 15	11 49·1	26	11 15	+ 43	31
11 42·2	5 35·6	21	11 44	11 45	27	11 44	+ 42·6	27

The following Table has been obtained by laying down the observed *points*, and drawing a curve amongst them by the hand, from which curve the interpolated *points* are obtained. The dotted line (see Plate I.) passes through the points observed, the continuous line, in the case of the London Docks, shows the curve which results from the theory of **Bernoulli**, (see Phil. Trans. 1831, Part II. p. 388.) The agreement between theory and observation so far amounts to identity, and is very remarkable.

The following Table shows the interval between the moon's transit and the corresponding high water at Brest, Plymouth, Portsmouth, Sheerness, and the London Docks.

Moon's Transit.	Brest.	Plymouth.	Portsmouth.	Sheerness.	London Docks.	Moon's Transit.
h m	h m	h m	h m	h m	h m	h m
0 0	3 48	5 33	11 40	+0 39	1 57	0 0
0 30	3 41	5 28	11 34	+0 34	1 50	0 30
1 0	3 33	5 22	11 28	+0 26	1 42	1 0
1 30	3 26	5 13	11 21	+0 17	1 35	1 30
2 0	3 18	5 4	11 14	+0 9	1 26	2 0
2 30	3 11	4 55	11 6	0 0	1 18	2 30
3 0	3 4	4 45	10 58	−0 9	1 11	3 0
3 30	2 58	4 37	10 52	−0 15	1 3	3 30
4 0	2 53	4 28	10 47	−0 20	0 56	4 0
4 30	2 50	4 21	10 44	−0 26	0 51	4 30
5 0	2 49	4 16	10 41	−0 31	0 45	5 0
5 30	2 50	4 13	10 42	−0 34	0 43	5 30
6 0	2 55	4 12	10 45	−0 35	0 42	6 0
6 30	3 5	4 18	10 54	−0 32	0 44	6 30
7 0	3 18	4 30	11 10	−0 22	0 52	7 0
7 30	3 33	4 51	11 26	−0 8	1 5	7 30
8 0	3 47	5 11	11 39	+0 9	1 22	8 0
8 30	3 58	5 26	11 51	+0 30	1 39	8 30
9 0	4 5	5 37	11 59	+0 44	1 56	9 0
9 30	4 8	5 45	12 2	+0 52	2 5	9 30
10 0	4 7	5 48	12 1	+0 54	2 10	10 0
10 30	4 5	5 47	11 58	+0 52	2 10	10 30
11 0	4 0	5 44	11 53	+0 48	2 8	11 0
11 30	3 54	5 38	11 47	+0 44	2 3	11 30

The preceding Table gives the principal or semi-menstrual inequality; the form of the curve is better seen by reference to the annexed plate, (see Plate I.) It appears that the *establishments* of Brest, Plymouth, Portsmouth, Sheerness and the London Docks are as follows:

Brest	3^h	48^m
Plymouth	5	33
Portsmouth	11	40
Sheerness	0	39
London Docks	1	57

Since this paper was presented to the Society, Mr. Dessiou has furnished me with the following results obtained by him from some observations communicated to the Society some time since by the late Mr. Fallows. The numbers are very irregular, owing to the paucity of observations and the difficulty of fixing the time of high water with precision, the *rise* being only about three feet. It appears that the *establishment* is about 2^h 56^m.

St. Helena.			St. Helena.		
Moon's Transit.	Corresponding Interval.	Number of Obs.	Moon's Transit.	Corresponding Interval.	Number of Obs.
h m	h m		h m	h m	
0 16	2 48	15	6 16	2 25	13
0 40	2 47	11	6 43	2 22	12
1 16	2 50	12	7 15	2 48	15
1 45	2 38	12	7 39	2 32	14
2 17	2 41	14	8 13	2 47	13
2 46	2 32	14	8 45	2 42	12
3 14	2 30	13	9 14	2 50	12
3 43	2 26	13	9 42	3 10	14
4 16	2 5	15	10 17	3 8	14
4 47	2 8	10	10 46	3 5	10
5 15	2 28	14	11 16	2 58	12
5 46	2 16	12	11 46	3 2	12

IV. *Experimental Researches in Electricity.—Third Series.* *By* MICHAEL FARADAY, *D.C.L. F.R.S. M.R.I. Fullerian Prof. Chem. Royal Institution, Corr. Mem. Royal Acad. of Sciences, Paris, Petersburgh, &c. &c.*

Read January 10th and 17th, 1833.

§ 7. *Identity of Electricities derived from different sources.* § 8. *Relation by measure of common and voltaic Electricity.*

§ 7. *Identity of Electricities derived from different sources.*

265. THE progress of the electrical researches which I have had the honour to present to the Royal Society, brought me to a point at which it was essential for the further prosecution of my inquiries that no doubt should remain of the identity or distinction of electricities excited by different means. It is perfectly true that CAVENDISH *, WOLLASTON †, COLLADON ‡ and others, have in turn removed some of the greatest objections to the acknowledgement of the identity of common, animal and voltaic electricity, and I believe that philosophers generally consider these electricities as really the same. But on the other hand it is also true, that the accuracy of WOLLASTON's experiments has been denied §, and that one of them, which really is no proof of chemical decomposition by common electricity (309. 327.), has been that selected by several experimenters as the test of chemical action (336. 346.). It is a fact, too, that many philosophers are still drawing distinctions between the electricities from different sources; or at least doubting whether their identity is proved. Sir HUMPHRY DAVY, for instance, in his paper on the Torpedo ‖, thought it probable

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* Phil. Trans. 1776, p. 196.

† Ibid. 1801, p. 434.

‡ Annales de Chimie, 1826, p. 62, &c.

§ Phil. Trans. 1832, p. 282, note.

‖ Phil. Trans. 1829, p. 17. "Common electricity is excited upon non-conductors, and is readily carried off by conductors and imperfect conductors. Voltaic electricity is excited upon combinations that animal electricity would be found of a peculiar kind; and referring to that, in association with common electricity, voltaic electricity and magnetism, has said, "Distinctions might be established in pursuing the various modifications or properties of electricity in these different forms, &c." Indeed I need only refer to the last volume of the Philosophical Transactions to show that the question is by no means considered as settled *.

266. Notwithstanding, therefore, the general impression of the identity of electricities, it is evident that the proofs have not been sufficiently clear and distinct to obtain approbation from all those who were competent to consider the subject; and the question seemed to me very much in the condition of that which Sir H. Davy solved so beautifully,—namely, whether voltaic electricity in all cases merely eliminated, or did not in some actually produce, the acid and alkali found after its action upon water. The same necessity that urged him to decide the doubtful point, which interfered with the extension of his views, and destroyed the strictness of his reasoning, has obliged me to

of perfect and imperfect conductors, and is only transmitted by perfect conductors or imperfect conductors of the best kind. Magnetism, if it be a form of electricity, belongs only to perfect conductors; and, in its modifications, to a peculiar class of them [1]. Animal electricity resides only in the imperfect conductors forming the organs of living animals, &c."

* Phil. Trans. 1832, p. 259. Dr. Davy, in making experiments on the torpedo, obtains effects the same as those produced by common and voltaic electricity, and says that in its magnetic and chemical power it does not seem to be essentially peculiar,—p. 274; but he then says, p. 275, there are other points of difference; and after referring to them, adds, "How are these differences to be explained? Do they admit of explanation similar to that advanced by Mr. Cavendish in his theory of the torpedo; or may we suppose, according to the analogy of the solar ray, that the electrical power, whether excited by the common machine, or by the voltaic battery, or by the torpedo, is not a simple power, but a combination of powers, which may occur variously associated, and produce all the varieties of electricity with which we are acquainted?"

At p. 279 of the same volume of Transactions is Dr. Ritchie's paper, from which the following are extracts: "Common electricity is diffused over the surface of the metal;—voltaic electricity exists within the metal. Free electricity is conducted over the surface of the thinnest gold leaf as effectually as over a mass of metal having the same surface;—voltaic electricity requires thickness of metal for its conduction," p. 280: and again, "The supposed analogy between common and voltaic electricity, which was so eagerly traced after the invention of the pile, completely fails in this case, which was thought to afford the most striking resemblance," p. 291.

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[1] Dr. Ritchie has shown this is not the case, Phil. Trans. 1832, p. 294.

ascertain the identity or difference of common and voltaic electricity. I have satisfied myself that they are identical, and I hope the proofs I have to offer, and the results flowing from them, will be found worthy the attention of the Royal Society.

267. The various phenomena exhibited by electricity may, for the purposes of comparison, be arranged under two heads; namely, those connected with electricity of tension, and those belonging to electricity in motion. This distinction is taken at present not as philosophical, but merely as convenient. The effect of electricity of tension, at rest, is either attraction or repulsion at sensible distances. The effects of electrical currents may be considered as 1st, Evolution of heat; 2nd, Magnetism; 3rd, Chemical decomposition; 4th, Physiological phenomena; 5th, Spark. It will be my object to compare electricities from different sources, and especially common and voltaic electricities, by their power of producing these effects.

I. *Voltaic Electricity.*

268. *Tension.*—When a voltaic battery of 100 pairs of plates has its extremities examined by the ordinary electrometer, it is well known that they are found positive and negative, the gold leaves at the same extremity repelling each other, the gold leaves at different extremities attracting each other, even when half an inch or more of air intervenes.

269. That ordinary electricity is discharged by points with facility through air; that it is readily transmitted through highly rarefied air; and also through heated air, as for instance a flame; is due to its high tension. I sought, therefore, for similar effects in the discharge of voltaic electricity, using as a test of the passage of the electricity either the galvanometer or chemical action produced by the arrangement hereafter to be described (312. 316.)

270. The voltaic battery I had at my disposal consisted of 140 pairs of plates four inches square, with double coppers. It was insulated throughout, and diverged a gold leaf electrometer about one third of an inch. On endeavouring to discharge this battery by delicate points very nicely arranged and approximated, either in the air or in an exhausted receiver, I could obtain no indications of a current, either by magnetic or chemical action. In this, how-

ever, was found no point of discordance between voltaic and common electricity; for when a Leyden battery (291.) was charged so as to deflect the gold leaf electrometer equally, the points were found equally unable to discharge it with such effect as to produce either magnetic or chemical action. This was not because common electricity could not produce both these effects (307. 310.), but because when of such low intensity the quantity required to make the effects visible (being enormously great (371. 375.),) could not be transmitted in any reasonable time. In conjunction with the proofs of identity hereafter to be given, these effects of points also prove identity instead of difference between voltaic and common electricity.

271. As heated air discharges common electricity with far greater facility than points, I hoped that voltaic electricity might in this way also be discharged. An apparatus was therefore constructed (Plate II. fig. 5.), in which A B is an insulated glass rod upon which two copper wires, C, D, are fixed firmly; to these copper wires are soldered two pieces of fine platina wire, the ends of which are brought very close to each other at *e*, but without touching; the copper wire C was connected with the positive pole of a voltaic battery, and the wire D with a decomposing apparatus (312. 316.), from which the communication was completed to the negative pole of the battery. In these experiments only two troughs, or twenty pairs of plates, were used.

272. Whilst in the state described, no decomposition took place at the point *a*, but when the side of a spirit-lamp flame was applied to the two platina extremities at *e*, so as to make them bright red-hot, decomposition occurred; iodine soon appeared at the point *a*, and the transference of electricity through the heated air was established. On raising the temperature of the points *e* by a blowpipe, the discharge was rendered still more free, and decomposition took place instantly. On removing the source of heat, the current immediately ceased. On putting the ends of the wires very close by the side of and parallel to each other, but not touching, the effects were perhaps more readily obtained than before. On using a larger voltaic battery (270.), they were also more freely obtained.

273. On removing the decomposing apparatus and interposing a galvanometer instead, heating the points *e* as the needle would swing one way, and removing the heat during the time of its return (302.), feeble deflections were

Fig. 4.

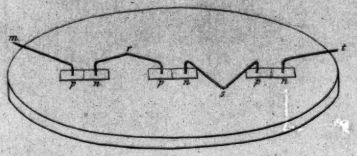
m
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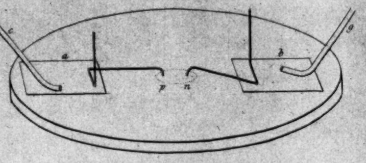
Fig. 2.



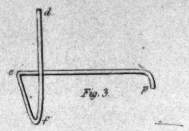


Fig. 3.

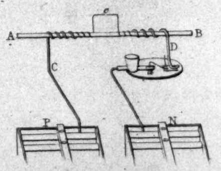


Fig. 5.

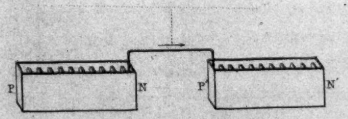


Fig. 1.

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soon obtained: thus also proving the current through heated air; but the instrument used was not so sensible under the circumstances as chemical action.

274. These effects, not hitherto known or expected under this form, are only cases of the discharge which takes place through air between the charcoal terminations of the poles of a powerful battery, when they are gradually separated after contact. Here the passage is through heated air exactly as with common electricity, and Sir H. DAVY has recorded that with the original battery of the Royal Institution this discharge passed through at least four inches of air *. In the exhausted receiver the electricity would *strike* through nearly half an inch of space, and the combined effects of rarefaction and heat was such upon the inclosed air as to enable it to conduct the electricity through a space of six or seven inches.

275. The instantaneous charge of a Leyden battery by the poles of a voltaic apparatus is another proof of the tension, and also the quantity, of electricity evolved by the latter. Sir H. DAVY says †, "When the two conductors from the ends of the combination were connected with a Leyden battery, one with the internal, the other with the external coating, the battery instantly became charged, and on removing the wires and making the proper connexions, either a shock or a spark could be perceived: and the least possible time of contact was sufficient to renew the charge to its full intensity."

276. *In motion;* i. *Evolution of Heat.*—The evolution of heat in wires and fluids by the voltaic current is matter of general notoriety.

277. ii. *Magnetism.*—No fact is better known to philosophers than the power of the voltaic current to deflect the magnetic needle, and to make magnets according to *certain laws;* and no effect can be more distinctive of an electrical current.

278. iii. *Chemical decomposition.*—The chemical powers of the voltaic current, and their subjection to *certain laws,* are also perfectly well known.

279. iv. *Physiological effects.*—The power of the voltaic current, when strong, to shock and convulse the whole animal system, and when weak to affect the tongue and the eyes, is very characteristic.

280. v. *Spark.*—The brilliant star of light produced by the discharge of a

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* Elements of Chemical Philosophy, p. 153. † Ibid. p. 154.

voltaic battery is known to all as the most beautiful light that man can produce by art.

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281. That these effects may be almost infinitely varied, some being exalted whilst others are diminished, is universally acknowledged; and yet without any doubt of the identity of character of the voltaic currents thus made to differ in their effect. The beautiful explication of these variations afforded by CAVENDISH's theory of quantity and intensity requires no support at present, as it is not understood to be doubted.

282. In consequence of the comparisons that will hereafter arise between wires carrying voltaic and ordinary electricities, and also because of certain views of the condition of a wire or any other conducting substance connecting the poles of a voltaic apparatus, it will be necessary to give some definite view of what is called the voltaic current, in contradistinction to any supposed peculiar state of arrangement, not progressive, which the wire or the electricity within it may be supposed to assume. If two voltaic troughs P N, P′ N′, fig. 1. be symmetrically arranged and insulated, and the ends N P′ connected by a wire, over which a magnetic needle is suspended, the wire will exert no effect over the needle; but immediately that the ends P N′ are connected by another wire, the needle will be deflected, and will remain so as long as the circuit is complete. Now if the troughs merely act by causing a peculiar arrangement in the wire either of its particles or its electricity, that arrangement constituting its electrical and magnetic state, then the wire N P′ should be in a similar state of arrangement *before* P and N′ were connected, to what it is afterwards, and should have deflected the needle, although less powerfully, perhaps to one half the extent which would result when the communication is complete throughout. But if the magnetic effects depend upon a current, then it is evident why they could not be produced in *any* degree before the circuit was complete; because prior to that no current could exist.

283. By *current*, I mean anything progressive, whether it be a fluid of electricity, or two fluids moving in opposite directions, or merely vibrations, or, speaking still more generally, progressive forces. By *arrangement*, I understand a local adjustment of particles, or fluids, or forces, not progressive. Many other reasons might be urged in support of the view of a *current* rather than

an *arrangement*, but I am anxious to avoid dilating unnecessarily upon what can be supplied by others at the moment.

II. *Ordinary Electricity.*

284. By ordinary electricity I understand that which can be obtained from the common machine, or from the atmosphere, or by pressure, or cleavage of crystals, or by a multitude of other operations; its distinctive character being that of great intensity, and the exertion of attractive and repulsive powers, not merely at sensible but at considerable distances.

285. *Tension.* The attractions and repulsions at sensible distances, caused by ordinary electricity, are well known to be so powerful in certain cases, as to surpass, almost infinitely, the similar phenomena produced by electricity, considered as of other kinds. But still those attractions and repulsions are exactly of the same nature as those already referred to under the head *Tension, Voltaic electricity* (268.); and the difference in degree between them is not greater than often occurs between cases of ordinary electricity only. I think it will be unnecessary to enter minutely into the proofs of the identity of this character in the two instances. They are abundant; are generally admitted as good; and lie upon the surface of the subject: and whenever in other parts of the comparison I am about to draw, a similar case occurs, I shall content myself with a mere announcement of the similarity, expanding only upon those parts where the great question of distinction or identity is still controverted.

286. The discharge of common electricity through heated air is a well-known fact. The parallel case of voltaic electricity has already been described (272, &c.).

287. *In motion.* i. *Evolution of heat.* The heating power of common electricity when passed through wires or other substances, is perfectly well known. The accordance between it and voltaic electricity is in this respect complete. Mr. Harris has constructed and described* a very beautiful and sensible instrument on this principle, in which the heat produced in a wire by the discharge of a mere spark of common electricity is readily shown, and to which I

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* Philosophical Transactions, 1827, p. 18. Edinburgh Transactions, 1831. Harris on a New Electrometer, &c. &c.

shall have occasion to refer for experimental proof in a future part of this paper (344.).

288. ii. *Magnetism.* Voltaic electricity has most extraordinary and exalted magnetic powers. If common electricity be identical with it, it ought to have the same powers. In rendering needles or bars magnetic, it is found to agree with voltaic electricity, and the *direction* of the magnetism, in both cases, is the same; but in deflecting the magnetic needle, it has been found deficient, so that sometimes the power has been denied altogether, and at other times distinctions have been hypothetically assumed for the purpose of avoiding the difficulty*.

289. M. COLLADON, of Geneva, considered that the difference might be due to the use of insufficient quantities of common electricity in all the experiments before made upon this point; and in a memoir read to the Academie des Sciences in 1826†, describes experiments, in which, by the use of a battery, points, and a delicate galvanometer, he succeeded in obtaining deflections, and thus establishing identity in that respect. MM. ARAGO, AMPERE, and SAVARY are mentioned in the paper as having witnessed a successful repetition of the experiments. But as no second witness of these effects has come forward, MM. ARAGO, AMPERE, and SAVARY, not having themselves published (that I am aware of,) their acceptance of the results, and as others have not been able to obtain the effects, M. COLLADON's conclusions have been by some doubted or denied; and an important point with me was to establish their accuracy, or remove them entirely from the received body of experimental evidence. I am happy to say that my results fully confirm those by M. COLLADON, and I should have had no occasion to describe them, but that they are essential as proofs of the accuracy of the final and general conclusions I am enabled to draw respecting the magnetic and chemical action of electricity, (360. 366. 367. 377. &c.).

290. The plate electrical machine I have used is fifty inches in diameter; it has two sets of rubbers; its prime conductor consists of two brass cylinders connected by a third, the whole length being twelve feet, and the surface in contact with air about 1422 square inches. When in good excitation, one

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* DEMONFERRAND's Manuel d'Electricité dynamique, p. 121.

† Annales de Chimie, xxxiii. p. 62.

revolution of the plate will give ten or twelve sparks from the conductors, each an inch in length. Sparks or flashes from ten to fourteen inches in length may easily be drawn from the conductors. Each turn of the machine, when worked moderately, occupies about ⅘ths of a second.

291. The electric battery consisted of fifteen equal jars. They are coated eight inches upwards from the bottom, and are twenty-three inches in circumference, so that each contains one hundred and eighty-four square inches of glass, coated on both sides, independent of the bottoms, which are thicker glass, and contain each about fifty square inches.

292. A good discharging train was arranged by connecting metallically a sufficiently thick wire with, first, the metallic gas pipes of the house, then with the metallic gas pipes belonging to the public gas works of London; and finally, with the metallic water pipes of London. It was so effectual as to carry off instantaneously electricity of the feeblest tension, even that of a single voltaic trough, and was essential to many of the experiments.

293. The galvanometer was one or the other of those formerly described (87. 205.), but the glass jar covering it and supporting the needle was coated inside and outside with tinfoil, and the upper part (left uncoated, that the motions of the needle might be examined,) was covered with a frame of wirework, having numerous sharp points projecting from it. When this frame and the two coatings were connected with the discharging train (292.), an insulated point or ball, connected with the machine when most active, might be brought within an inch of any part of the galvanometer, yet without affecting the needle within by any ordinary electrical attraction or repulsion.

294. In connexion with these precautions, it may be necessary to state that the needle of the galvanometer is very liable to have its magnetic power deranged, diminished, or even inverted by the passage of a shock through the instrument. If the needle be at all oblique in the wrong direction to the coils of the galvanometer, when the shock passes, effects of this kind are sure to happen.

295. It was to the retarding power of bad conductors, that I first looked with the hope of being able to make common electricity assume more of the characters and power of voltaic electricity, than it is usually supposed to have.

296. The coating and armour of the galvanometer were first connected with the discharging train (292.); the end B (87.) of the galvanometer wire was connected with the outside coating of the battery, and then both these with the discharging train; the end A of the galvanometer wire was connected with a discharging rod by a wet thread four feet long; and finally, when the battery had been positively charged by about forty turns of the machine, it was discharged by the rod and the thread through the galvanometer. The needle immediately moved.

297. During the time that the needle completed its vibration in the first direction and returned, the machine was worked, and the battery charged; and when the needle in vibrating resumed its first direction, the discharge was again made through the galvanometer. By repeating this action a few times, the vibrations soon extended to above 40° on each side of the line of quiescence.

298. This effect could be obtained at pleasure. Nor was it varied, apparently, either in direction or degree, by using a short thick string, or even four short thick strings in place of the long fine thread. With a more delicate galvanometer, an excellent swing of the needle could be obtained by one discharge of the battery.

299. On reversing the galvanometer communications so as to pass the discharge through from B to A, the needle was equally well deflected, but in the opposite direction.

300. The deflections were in the same direction as if a voltaic current had been passed through the galvanometer, i. e. the positively charged surface of the electric battery coincided with the positive end of the voltaic apparatus (268.), and the negative surface of the first with the negative end of the latter.

301. The battery was then thrown out of use, and the communications so arranged that the current could be passed from the prime conductor, by the discharging rod held against it, through the wet string, through the galvanometer coil, and into the discharging train (292.), by which it was finally dispersed. This current could be stopped at any moment, by removing the discharging rod, and either stopping the machine or connecting the prime conductor by another rod with the discharging train; and could be as instantly renewed. The needle was so adjusted, that whilst vibrating in moderate and small arcs, it required time equal to twenty-five beats of a watch to pass in one

direction through the arc, and of course an equal time to pass in the other direction.

302. Thus arranged, and the needle being stationary, the current, direct from the machine, was sent through the galvanometer for twenty-five beats, then interrupted for other twenty-five beats, renewed for twenty-five beats more, again interrupted for an equal time, and so on continually. The needle soon began to vibrate visibly, and after several alternations of this kind, the vibration increased to 40° or more.

303. On changing the direction of the current through the galvanometer, the direction of the deflection of the needle was also changed. In all cases the motion of the needle was the same in direction as that caused by the use of an electric battery or a voltaic trough (300.).

304. I now rejected the wet string, and substituted a copper wire, so that the electricity of the machine passed at once into wires communicating directly with the discharging train, the galvanometer coil being one of the wires used for the discharge. The effects were exactly those obtained above (302.).

305. Instead of passing the electricity through the system, by bringing the discharging rod at the end of it into contact with the conductor, four points were fixed on to the rod; when the current was to pass, they were held about twelve inches from the conductor, and when it was not to pass, they were turned away. Then, except with this variation, operating as before (302.), the needle was soon powerfully deflected, and in perfect consistency with the former results. Points afforded the means by which Colladon, in all cases, made his discharges.

306. Finally, I passed the electricity first through an exhausted receiver, so as to make it there resemble the aurora borealis, and then through the galvanometer to the earth; and it was found still effective in deflecting the needle, and apparently with the same force as before.

307. From all these experiments, it appears that a current of common electricity, whether transmitted through water or wire, or rarefied air, or by means of points in common air, is still able to deflect the needle; the only requisite being, apparently, to allow time for its action: that it is, in fact, just as magnetic in every respect as a voltaic current, and that in this character therefore no distinction exists.

308. Imperfect conductors, as water, brine, acids, &c. &c. will be found far more convenient for exhibiting these effects than other modes of discharge, as by points or balls; for the former convert at once the charge of a powerful battery into a feeble spark, or rather continuous current, and involve little or no risk of deranging the magnetism of the needles (294.).

309. iii. *Chemical decomposition.* The chemical action of voltaic electricity is characteristic of that agent, but not more characteristic than are the laws under which the bodies evolved by decomposition arrange themselves at the poles. Dr. Wollaston showed* that common electricity resembled it in these effects, and "that they are both essentially the same;" but he mingled with his proofs an experiment having a resemblance, and nothing more, to a case of voltaic decomposition, which however he himself partly distinguished; and this has been more frequently referred to by others, on the one hand, to prove the occurrence of electro-chemical decomposition, like that of the pile, and on the other to throw doubt upon the whole paper, than the more numerous and decisive experiments which he has detailed.

310. I take the liberty of describing briefly my results, and of thus adding my testimony to that of Dr. Wollaston on the identity of voltaic and common electricity as to chemical action, not only that I may facilitate the repetition of the experiments, but also lead to some new consequences respecting electro-chemical decomposition (376. 377.).

311. I first repeated Wollaston's fourth experiment†, in which the ends of coated silver wires are immersed in a drop of sulphate of copper. By passing the electricity of the machine through such an arrangement, that end in the drop which received the electricity became coated with metallic copper. One hundred turns of the machine produced an evident effect; two hundred turns a very sensible one. The decomposing action was however very feeble. Very little copper was precipitated, and no sensible trace of silver from the other pole appeared in the solution.

312. A much more convenient and effectual arrangement for chemical decompositions by common electricity, is the following. Upon a glass plate, fig. 2, placed over, but raised above a piece of white paper, so that shadows may not interfere, put two pieces of tinfoil *a*, *b*; connect one of these by an

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* Philosophical Transactions, 1801, p. 427, 434.

† Ibid. 1801, p. 429.

insulated wire *c*, or wire and string (301.) with the machine, and the other *g*, with the discharging train (292.) or the negative conductor; provide two pieces of fine platina wire, bent as in fig. 3, so that the part *d*, *f* shall be nearly upright, whilst the whole is resting on the three bearing points *p*, *e*, *f*; place these as in fig. 2; the points *p*, *n* then become the decomposing poles. In this way surfaces of contact, as minute as possible, can be obtained at pleasure, and the connexion can be broken or renewed in a moment, and the substances acted upon examined with the utmost facility.

313. A coarse line was made on the glass with solution of sulphate of copper, and the terminations *p* and *n* put into it; the foil *a* was connected with the positive conductor of the machine by wire and wet string, so that no sparks passed: twenty turns of the machine caused the precipitation of so much copper on the end *p*, that it looked like copper wire; no apparent change took place at *n*.

314. A mixture of half muriatic acid and half water was rendered deep blue by sulphate of indigo, and a large drop put on the glass, fig. 2, so that *p* and *n* were immersed at opposite sides; a single turn of the machine showed bleaching effects round *p*, from evolved chlorine. After twenty revolutions no effect of the kind was visible at *n*, but so much chlorine had been set free at *p*, that when the drop was stirred the whole became colourless.

315. A drop of solution of iodide of potassium mingled with starch was put into the same position at *p* and *n*; on turning the machine, iodine was evolved at *p*, but not at *n*.

316. A still further improvement in this form of apparatus consists in wetting a piece of filtering paper in the solution to be experimented on, and placing that under the points *p* and *n*, on the glass: the paper retains the substance evolved at the point of evolution, by its whiteness renders any change of colour visible, and allows of the point of contact between it and the decomposing wires being contracted to the utmost degree. A piece of paper moistened in the solution of iodide of potassium and starch, or of the iodide alone, with certain precautions (322.), is a most admirable test of electro-chemical action; and when thus placed and acted upon by the electric current, will show iodine evolved at *p* by only half a turn of the machine. With these adjustments and the use of iodide of potassium on paper, chemical action is

sometimes a more delicate test of electrical currents than the galvanometer (273.). Such cases occur when the bodies traversed by the current are bad conductors, or when the quantity of electricity evolved or transmitted in a given time is very small.

317. A piece of litmus paper moistened in solution of common salt or sulphate of soda, was quickly reddened at *p*. A similar piece moistened in muriatic acid was very soon bleached at *p*. No effects of a similar kind took place at *n*.

318. A piece of turmeric paper moistened in solution of sulphate of soda was reddened at *n* by two or three turns of the machine, and in twenty or thirty turns plenty of alkali was there evolved. On turning the paper round, so that the spot came under *p*, and then working the machine, the alkali soon disappeared, the place became yellow, and a brown alkaline spot appeared in the new part under *n*.

319. On combining a piece of litmus with a piece of turmeric paper, wetting both with solution of sulphate of soda, and putting the paper on the glass, so that *p* was on the litmus and *n* on the turmeric, a very few turns of the machine sufficed to show the evolution of acid at the former and alkali at the latter, exactly in the manner effected by a volta-electric current.

320. All these decompositions took place equally well, whether the electricity passed from the machine to the foil *a*, through water, or through wire only; by *contact* with the conductor, or by *sparks* there; provided the sparks were not so large as to cause the electricity to pass in sparks from *p* to *n*, or towards *n*; and I have seen no reason to believe that in cases of true electro-chemical decomposition by the machine, the electricity passed in sparks from the conductor, or at any part of the current, is able to do more, because of its tension, than that which is made to pass merely as a regular current.

321. Finally, the experiment was extended into the following form, supplying in this case the fullest analogy between common and voltaic electricity. Three compound pieces of litmus and turmeric paper (319.) were moistened in solution of sulphate of soda, and arranged on a plate of glass with platina wires, as in fig. 4. The wire *m* was connected with the prime conductor of the machine, the wire *t* with the discharging train, and the wires *r* and *s* en-

tered into the course of the electrical current by means of the pieces of moistened paper; they were so bent as to rest each on three points, *n*, *r*, *p*; *n*, *s*, *p*, the points *r* and *s* being supported by the glass, and the others by the papers: the three terminations *p*, *p*, *p* rested on the litmus, and the other three *n*, *n*, *n* on the turmeric paper. On working the machine for a short time only, acid was evolved at *all* the poles or terminations *p*, *p*, *p*, by which the electricity entered the solution, and alkali at the other poles *n*, *n*, *n*, by which the electricity left the solution.

322. In all experiments of electro-chemical decomposition by the common machine and moistened papers (316.), it is necessary to be aware of and to avoid the following important source of error. If a spark passes over moistened litmus and turmeric paper, the litmus paper (provided it be delicate and not too alkaline,) is reddened by it; and if several sparks are passed, it becomes powerfully reddened. If the electricity pass a little way from the wire over the surface of the moistened paper, before it finds mass and moisture enough to conduct it, then the reddening extends as far as the ramifications. If similar ramifications occur at the termination *n*, on the turmeric paper, they *prevent* the occurrence of the red spot due to the alkali, which would otherwise collect there; sparks or ramifications from the points *n* will also redden litmus paper. If paper moistened by a solution of iodide of potassium (which is an admirably delicate test of electro-chemical action,) be exposed to the sparks or ramifications, or even a feeble stream of electricity through the air from either the point *p* or *n*, iodine will be immediately evolved.

323. These effects must not be confounded with those due to the true electro-chemical powers of common electricity, and must be carefully avoided when the latter are to be observed. No sparks should be passed, therefore, in any part of the current, nor any increase of intensity allowed by which the electricity may be induced to pass between the platina wires and the moistened papers, otherwise than by conduction; for if it burst through the air, the effect referred to ensues.

324. The effect itself is due to the formation of nitric acid by the combination of the oxygen and nitrogen of the air, and is, in fact, only a delicate repetition of Cavendish's beautiful experiment. The acid so formed, though small in quantity, is in a high state of concentration as to water, and pro-

duces the consequent effects of reddening the litmus paper, or preventing the exhibition of alkali on the turmeric paper, or, by acting on the iodide of potassium, evolving iodine.

325. By moistening a very small slip of litmus paper in solution of caustic potassa, and then passing the electric spark over its length in the air, I gradually neutralized the alkali, and ultimately rendered the paper red; on drying it, I found that nitrate of potassa had resulted from the operation, and that the paper had become touch-paper.

326. Either litmus paper or white paper moistened in solution of iodide of potassium, offers therefore a very simple, beautiful, and ready means of illustrating CAVENDISH'S experiment of the formation of nitric acid from the atmosphere.

327. I have already had occasion to refer to an experiment (265. 309.) by Dr. WOLLASTON, which is insisted upon too much, both by those who oppose and those who agree with the accuracy of his views respecting the identity of voltaic and ordinary electricity. By covering fine wires with glass or other insulating substances, and then removing only so much matter as to expose the point, or a section of the wires, and by passing electricity through two such wires, the guarded points of which were immersed in water, WOLLASTON found that the water could be decomposed even by the current from the machine, without sparks, and that two streams of gas arose from the points, exactly resembling, in appearance, those produced by voltaic electricity, and, like the latter, giving a mixture of oxygen and hydrogen gases. But Dr. WOLLASTON himself points out that the effect is different from that of the voltaic pile, inasmuch as both oxygen and hydrogen are evolved from *each* pole; he calls it "a very close *imitation* of the galvanic phenomena," but adds, that "in fact the resemblance is not complete," and does not trust to it to establish the principles, correctly laid down in his paper.

328. This experiment is neither more nor less than a repetition, in a refined manner, of that made by Dr. PEARSON in 1797*, and previously by MM. PAETS VAN TROOSTWYK and DEIMAN in 1789 or earlier. That the experiment should never be quoted as proving true electro-chemical decomposition, is sufficiently evident from the circumstance, that the law which regulates the trans-

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* NICHOLSON'S Journal, 4to, vol. i. pp. 241, 299, 349.

ference and final place of the evolved bodies (278. 309.) has no influence here. The water is decomposed at both poles independently of each other, and the oxygen and hydrogen evolved at the wires are the elements of the water existing the instant before in those places. That the poles, or rather points, have no mutual decomposing dependence, may be shown by substituting a wire, or the finger, for one of them, a change which does not at all interfere with the other, though it stops all action at the changed pole. This fact may be observed by turning the machine for some time; for though bubbles will rise from the point left unaltered, in quantity sufficient to cover entirely the wire used for the other communication, if they could be applied to it, yet not a single bubble will appear on that wire.

329. When electro-chemical decomposition takes place, there is great reason to believe that the quantity of matter decomposed is not proportionate to the intensity, but to the quantity of electricity passed (320.). Of this I shall be able to offer some proofs in a future part of this paper (375. 377.). But in the experiment under consideration, this is not the case. If, with a constant pair of points, the electricity be passed from the machine in sparks, a certain proportion of gas is evolved; but if the sparks be rendered shorter, less gas is evolved, and if no sparks be passed, there is scarcely a sensible portion of gases set free. On substituting solution of sulphate of soda for water, scarcely a sensible quantity of gas could be procured even with powerful sparks, and almost none with the mere current; yet the quantity of electricity in a given time was the same in all these cases.

330. I do not intend to deny that with such an apparatus common electricity can decompose water in a manner analogous to that of the voltaic pile; I believe at present that it can. But when what I consider the true effect only was obtained, the quantity of gas given off was so small that I could not ascertain whether it was, as it ought to be, oxygen at one wire and hydrogen at the other. Of the two streams one seemed more copious than the other, and on turning the apparatus round, still the same side in relation to the machine gave the largest stream. On substituting solution of sulphate of soda for pure water (329.), these minute streams were still observed. But the quantities were so small, that on working the machine for half an hour I could not obtain at either pole a bubble of gas larger than a small grain of sand. If the

conclusion which I have drawn (377.) relating to the amount of chemical action be correct, this ought to be the case.

331. I have been the more anxious to assign the true value of this experiment as a test of electro-chemical action, because I shall have occasion to refer to it in cases of supposed chemical action by magneto-electric and other electric currents (336. 346.) and elsewhere. But, independent of it, there cannot be now a doubt that Dr. Wollaston was right in his general conclusion; and that voltaic and common electricity have powers of chemical decomposition, alike in their nature, and governed by the same law of arrangement.

332. iv. *Physiological effects.*—The power of the common electric current to shock and convulse the animal system, and when weak to affect the tongue and the eyes, may be considered as the same with the similar power of voltaic electricity, account being taken of the intensity of the one electricity and duration of the other. When a wet thread was interposed in the course of the current of common electricity from the battery (291.) charged by eight or ten revolutions of the machine in good action (290.), and the discharge made by platina spatulas through the tongue or the gums, the effect upon the tongue and eyes was exactly that of a feeble voltaic circuit.

333. v. *Spark.*—The beautiful flash of light attending the discharge of common electricity is well known. It rivals in brilliancy, if it does not even very much surpass, the light from the discharge of voltaic electricity; but it endures for an instant only, and is attended by a sharp noise like that of a small explosion. Still no difficulty can arise in recognising it to be the same spark as that from the voltaic battery, especially under certain circumstances. The eye cannot distinguish the difference between a voltaic and a common electricity spark, if they be taken between amalgamated surfaces of metal, at intervals only, and through the same distance of air.

334. When the battery (291.) was discharged through a wet string placed in some part of the circuit away from the place where the spark was to pass, the spark was yellowish, flamy, having a duration sensibly longer than if the water had not been interposed, was about three fourths of an inch in length, was accompanied by little or no noise, and whilst losing part of its usual character had approximated in some degree to the voltaic spark. When the electricity retarded by water was discharged between pieces of charcoal, it was

exceedingly luminous and bright upon both surfaces of the charcoal, resembling the brightness of the voltaic discharge on such surfaces. When the discharge of the unretarded electricity was taken upon charcoal, it was bright upon both the surfaces, (in that respect resembling the voltaic spark,) but the noise was loud, sharp and ringing.

335. I have assumed, in accordance, I believe, with the opinion of every other philosopher, that atmospheric electricity is of the same nature with ordinary electricity (284.), and I might therefore refer to certain published statements of chemical effects produced by the former as proofs that the latter enjoys the power of decomposition in common with voltaic electricity. But the comparison I am drawing is far too rigorous, to allow me to use these statements without being fully assured of their accuracy; and I have no right to suppress them, because, if accurate, they establish what I am labouring to put on an undoubted foundation, and have priority to my results.

336. M. Bonijol of Geneva [*] is said to have constructed very delicate apparatus for the decomposition of water by common electricity. By connecting an insulated lightning rod with this apparatus, the decomposition of the water proceeded in a continuous and rapid manner even when the electricity of the atmosphere was not very powerful. The apparatus is not described; but as the diameter of the wire is mentioned as very small, it appears to have been similar in construction to that of Wollaston (327.); and as that does not furnish a case of true polar electro-chemical decomposition (328.), this result of M. Bonijol does not prove the identity in chemical action of common and voltaic electricity.

337. At the same page of the Bibliotheque Universelle, M. Bonijol is said to have decomposed *potash*, and also chloride of silver, by putting them into very narrow tubes and passing electric sparks from an ordinary machine over them. It is evident that these offer no analogy to cases of true voltaic decomposition, where the electricity only decomposes when it is *conducted* by the body acted upon, and ceases to decompose, according to its ordinary laws, when it passes in sparks. These effects are probably partly analogous to that which takes place with water in Pearson's or Wollaston's apparatus, and may be due to very high temperature acting on minute portions of matter; or they may be

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[*] Bibliotheque Universelle 1830, tome xlv. p. 213.

connected with the results in air (322.). As nitrogen can combine directly with oxygen under the influence of the electric spark (324.), it is not impossible that it should even take it from the potassium of the potash, especially as there would be plenty of potassa in contact with the acting particles to combine with the nitric acid formed. However distinct all these actions may be from true polar electro-chemical decompositions, they are still highly important, and well worthy of investigation.

338. The late Mr. Barry communicated a paper to the Royal Society[*] last year, so distinct in the details that it would seem at once to prove the identity in chemical action of common and voltaic electricity, but that, when examined, considerable difficulty arises in reconciling certain of the effects with the remainder. He used two tubes, each having a wire within it passing through the closed end, as is usual for voltaic decompositions. The tubes were filled with solution of sulphate of soda, coloured with syrup of violets, and connected by a portion of the same solution, in the ordinary manner; the wire in one tube was connected by a *gilt thread* with the string of an insulated electrical kite, and the wire in the other tube by a similar *gilt thread* with the ground. Hydrogen soon appeared in the tube connected with the kite, and oxygen in the other, and in ten minutes the liquid in the first tube was green from the alkali evolved, and that in the other red from free acid produced. The only indication of the strength of the atmospheric electricity is in the expression, "the usual shocks were felt on touching the string."

339. That the electricity in this case does not resemble that from any ordinary source of common electricity, is shown by several circumstances. Wollaston could not effect the decomposition of water by such an arrangement, and obtain the gases in *separate* vessels, using common electricity; nor have any of the numerous philosophers, who have employed such an apparatus, obtained any such decomposition, either of water or of a neutral salt, by the use of the machine. I have lately tried the large machine (290.) in full action for a quarter of an hour, during which seven hundred revolutions were made without producing any sensible effects, although the shocks that it would then give must have been far more powerful and numerous than could have been taken, with any chance of safety, from an electrical kite-string; and by reference

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[*] Philosophical Transactions, 1831, p. 165.

to the comparison hereafter to be made (371.), it will be seen that for common electricity to have produced the effect, the quantity must have been awfully great, and apparently far more than could have been conducted to the earth by a gilt thread, and at the same time only have produced the "usualshocks."

340. That the electricity was apparently not analogous to voltaic electricity is evident, for the "usual shocks" only were produced, and nothing like the terrible sensation due to a voltaic battery, even when it has a tension so feeble as not to strike through the eighth of an inch of air.

341. It seems just possible that the air which was passing by the kite and string, being in an electrical state sufficient to produce the "usual shocks" only, could still, when the electricity was drawn off below, renew the charge, and so continue the current. The string was 1500 feet long, and contained two double threads. But when the enormous quantity which must have been thus collected is considered (371. 376.), the explanation seems very doubtful. I charged a voltaic battery of twenty pairs of plates four inches square and with double coppers, very strongly, insulated it, connected its positive extremity with the discharging train (292.), and its negative pole with an apparatus like that of Mr. Barry, communicating by a wire inserted three inches into the wet soil of the ground. This battery thus arranged produced feeble decomposing effects, as nearly as I could judge answering the description Mr. Barry has given. Its intensity was, of course, far lower than the electricity of the kite-string, but the supply of quantity from the discharging train was unlimited. It of course gave no shocks to compare with the "usual shocks" of a kite-string.

342. Mr. Barry's experiment is a very important one to repeat and verify. If confirmed, it will be, as far as I am aware, the first recorded case of true electro-chemical decomposition of water by common electricity, and it will supply a form of electrical current, which, both in quantity and intensity, is exactly intermediate between those of the common electrical machine and the voltaic pile.

III. *Magneto-Electricity.*

343. *Tension.*—The attractions and repulsions due to the tension of electricity have been well observed with that evolved by magneto-electric induction. M. Pixii, by using an apparatus, clever in its construction and powerful in its

action[*], was able to obtain great divergence of the gold leaves of an electrometer[†].

344. *In motion :* i. *Evolution of Heat.*—The current produced by magneto-electric induction, can heat a wire in the manner of ordinary electricity. At the British Association of Science at Oxford, in June of the present year, I had the pleasure, in conjunction with Mr. Harris, Professor Daniell, Mr. Duncan, and others, of making an experiment, for which the great magnet in the museum, Mr. Harris's new electrometer (287.), and the magneto-electric coil described in my first paper (34.), were put in requisition. The latter had been modified in the manner I have elsewhere described[‡], so as to produce an electric spark when its contact with the magnet was made or broken. The terminations of the spiral, adjusted so as to have their contact with each other broken when the spark was to pass, were connected with the wire in the electrometer, and it was found that each time the magnetic contact was made and broken, expansion of the air within the instrument occurred, indicating an increase, at the moment, of the temperature of the wire.

345. ii. *Magnetism.*—These currents were discovered by their magnetic power.

346. iii. *Chemical decomposition.*—I have made many endeavours to effect chemical decomposition by magneto-electricity, but unavailingly. In July last I received an anonymous letter (which has since been published[§],) describing a magneto-electric apparatus, by which the decomposition of water was effected. As the term "guarded points" is used, I suppose the apparatus to have been Wollaston's (327. &c.), in which case the results did not indicate polar electro-chemical decomposition. Signor Botto has recently published certain results which he has obtained[‖]; but they are, as at present described, inconclusive. The apparatus he used was apparently that of Dr. Wollaston, which gives only fallacious indications (327. &c.). As magneto-electricity can produce sparks, it would be able to show the effects proper to this apparatus. The apparatus of M. Pixii already referred to (343.) has however, in the hands of himself[¶] and M. Hachette[**], given decisive chemical results, so as to com-

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[*] Annales de Chimie, l. p. 322.

[†] Ibid. li. p. 77.

[‡] Phil. Mag. and Annals, 1832, vol. xi. p. 405.

[§] Lond. and Edinb. Phil. Mag. and Journ. 1832, vol. i. p. 161.

[‖] Ibid. 1832, vol. i. p. 441.

[¶] Annales de Chimie, li. p. 77.

[**] Ibid. li. p. 72.

plete this link in the chain of evidence. Water was decomposed by it, and the oxygen and hydrogen obtained in separate tubes according to the law governing volta-electric and machine-electric decomposition.

347. iv. *Physiological effects.*—A frog was convulsed in the earliest experiments on these currents (56.). The sensation upon the tongue, and the flash before the eyes, which I at first obtained only in a feeble degree (56.), have been since exalted by more powerful apparatus, so as to become even disagreeable.

348. v. *Spark.*—The feeble spark which I first obtained with these currents (32.), has been varied and strengthened by Signori NOBILI and ANTINORI, and others, so as to leave no doubt as to its identity with the common electric spark.

IV. *Thermo-Electricity.*

349. With regard to thermo-electricity, (that beautiful form of electricity discovered by SEEBECK,) the very conditions under which it is excited are such as to give no ground for expecting that it can be raised like common electricity to any high degree of tension; the effects, therefore, due to that state are not to be expected. The sum of evidence respecting its analogy to the electricities already described, is, I believe, as follows:—*Tension.* The attractions and repulsions due to a certain degree of tension have not been observed. *In currents:* i. *Evolution of Heat.* I am not aware that its power of raising temperature has been observed. ii. *Magnetism.* It was discovered, and is best recognised, by its magnetic powers. iii. *Chemical decomposition* has not been effected by it. iv. *Physiological effects.* NOBILI has shown* that these currents are able to cause contractions in the limbs of a frog. v. *Spark.* The spark has not yet been seen.

350. Thus only those effects are weak or deficient, which depend upon a certain high degree of intensity; and if common electricity be reduced in that quality to a similar degree with the thermo-electricity, it can produce no effects beyond the latter.

V. *Animal Electricity.*

351. After an examination of the experiments of WALSH†, INGENHOUSZ‡,

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[*] Bibliotheque Universelle, xxxvii. 15.

[†] Philosophical Transactions, 1773, p. 461.

[‡] Phil. Trans. 1775, p. 1.

Cavendish[*], Sir H. **Davy**[†], and Dr. **Davy**[‡], no doubt remains on my mind as to the identity of the electricity of the torpedo with common and voltaic electricity; and I presume that so little will remain on the minds of others as to justify my refraining from entering at length into the philosophical proofs of that identity. The doubts raised by Sir H. **Davy** have been removed by his brother Dr. **Davy**; the results of the latter being the reverse of those of the former. At present the sum of evidence is as follows:—

352. *Tension.*—No sensible attractions or repulsions due to tension have been observed.

353. *In motion:* i. *Evolution of Heat*; not yet observed: I have little or no doubt that **Harris**'s electrometer would show it (287. 359.).

354. ii. *Magnetism.*—Perfectly distinct. According to Dr. **Davy**[§], the current deflected the needle and made magnets under the same law, as to direction, which governs currents of ordinary and voltaic electricity.

355. iii. *Chemical decomposition.*—Also distinct; and though Dr. **Davy** used an apparatus of similar construction with that of Dr. **Wollaston** (327.), still no error in the present case is involved, for the decompositions were polar, and in their nature truly electro-chemical. By the direction of the magnet, it was found that the under surface of the fish was negative, and the upper positive; and in the chemical decompositions, silver and lead were precipitated on the wire connected with the under surface, and not on the other; and when these wires were either steel or silver, in solution of common salt, gas (hydrogen?) rose from the negative wire, but none from the positive.

356. Another reason for the decomposition being electro-chemical is, that a **Wollaston**'s apparatus constructed with *wires*, coated by sealing-wax, would most probably not have decomposed water, even in its own peculiar way, without the electricity rising high enough in intensity to produce sparks in some part of the circuit; whereas the torpedo was not able to produce sensible sparks. A third reason is, that the purer the water in **Wollaston**'s apparatus, the more abundant is the decomposition: and I have found that a machine and wire points which succeeded perfectly well with distilled water, failed altogether when the water was rendered a good conductor by sulphate of soda, common

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[*] Philosophical Transactions, 1776, p. 196.

[†] Ibid. 1829, p. 15.

[‡] Ibid. 1832, p. 259.

[§] Ibid. 1832, p. 260.

salt, or other saline bodies. But in Dr. Davy's experiments with the torpedo, *strong* solutions of salt, nitrate of silver, and superacetate of lead were used successfully, and there is no doubt with more success than weaker ones.

357. iv. *Physiological effects.*—These are so characteristic, that by them the peculiar powers of the torpedo and gymnotus are principally recognised.

358. v. *Spark.*—The electric spark has not yet been obtained, or at least I think not; but perhaps I had better refer to the evidence on this point. Humboldt, speaking of results obtained by M. Fahlberg, of Sweden, says, "This philosopher has seen an electric spark, as Walsh and Ingenhousz had done before him at London, by placing the gymnotus in the air, and interrupting the conducting chain by two gold leaves pasted upon glass, and a line distant from each other*." I cannot, however, find any record of such an observation by either Walsh or Ingenhousz, and do not know where to refer to that by M. Fahlberg. M. Humboldt could not himself perceive any luminous effect.

Again, Sir John Leslie, in his dissertation on the progress of mathematical and physical science, prefixed to the seventh edition of the Encyclopædia Britannica, Edinb. 1830, p. 622, says, "From a healthy specimen" of the *Silurus electricus*, meaning rather the *gymnotus*, "exhibited in London, vivid sparks were drawn in a darkened room;" but he does not say he saw them himself, nor state who did see them; nor can I find any account of such a phenomenon; so that the statement is doubtful†.

359. In concluding this summary of the powers of torpedinal electricity, I cannot refrain from pointing out the enormous absolute quantity of electricity which the animal must put in circulation at each effort. It is doubtful whether any common electrical machine has as yet been able to supply electricity sufficient in a reasonable time to cause true electro-chemical decomposition of water (330. 339.), yet the current from the torpedo has done it. The same high proportion is shown by the magnetic effects (296. 371.). These circumstances indicate that the torpedo has power (in the way probably that Cavendish describes,) to continue the evolution for a sensible time, so that its suc-

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* Edinburgh Phil. Journal, ii. p. 249.

† Mr. Brayley, who referred me to these statements, and has extensive knowledge of recorded facts, is unacquainted with any further account relating to them.

cessive discharges rather resemble those of a voltaic arrangement, intermitting in its action, than those of a Leyden apparatus, charged and discharged many times in succession. In reality, however, there is *no philosophical difference* between these two cases.

360. The *general conclusion* which must, I think, be drawn from this collection of facts is, that *electricity, whatever may be its source, is identical in its nature.* The phenomena in the five kinds or species quoted, differ not in their character, but only in degree; and in that respect vary in proportion to the variable circumstances of *quantity* and *intensity** which can at pleasure be made to change in almost any one of the kinds of electricity, as much as it does between one kind and another.

Table of the experimental Effects common to the Electricities derived from different Sources.

	Physiological Effects.	Magnetic Deflection.	Magnets made.	Spark.	Heating Power.	True chemical Action.	Attraction and Repulsion.	Discharge by Hot Air.
1. Voltaic Electricity....	×	×	×	×	×	×	×	×
2. Common Electricity ..	×	×	×	×	×	×	×	×
3. Magneto-Electricity ..	×	×	×	×	×	×	×	
4. Thermo-Electricity ..	×	×	?		?			
5. Animal Electricity....	×	×	×	?		×		

§ 8. *Relation by Measure of common and voltaic Electricity.*

361. Believing the point of identity to be satisfactorily established, I next endeavoured to obtain a common measure, or a known relation as to quantity, of the electricity excited by a machine, and that from a voltaic pile; for the purpose not only of confirming their identity (378.), but also of demonstrating certain general principles (366. 377, &c.), and creating an extension of the means of investigating and applying the chemical powers of this wonderful and subtile agent.

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* The term *quantity* in electricity is perhaps sufficiently definite as to sense; the term *intensity* is more difficult to define strictly. I am using the terms in their ordinary and at present accepted meaning.

362. The first point to be determined was, whether the same absolute quantity of ordinary electricity, sent through a galvanometer, under different circumstances, would cause the same deflection of the needle. An arbitrary scale was therefore attached to the galvanometer, each division of which was equal to about 4°, and the instrument arranged as in former experiments (296.). The machine (290.), battery (291.), and other parts of the apparatus were brought into good order, and retained for the time as nearly as possible in the same condition. The experiments were alternated so as to indicate any change in the condition of the apparatus and supply the necessary corrections.

363. Seven of the battery jars were removed, and eight retained for present use. It was found that about forty turns would fully charge the eight jars. They were then charged by thirty turns of the machine, and discharged through the galvanometer, a thick wet string, about ten inches long, being included in the circuit. The needle was immediately deflected five divisions and a half, on the one side of the zero, and in vibrating passed as nearly as possible through five divisions and a half on the other side.

364. The other seven jars were then added to the eight, and the whole fifteen charged by thirty turns of the machine. The HENLEY's electrometer stood not quite half as high as before; but when the discharge was made through the galvanometer, previously at rest, the needle immediately vibrated, passing *exactly* to the same division as in the former instance. These experiments with eight and with fifteen jars were repeated several times alternately with the same results.

365. Other experiments were then made, in which all the battery was used, and its charge (being fifty turns of the machine,) sent through the galvanometer: but it was modified by being passed sometimes through a mere wet thread, sometimes through thirty-eight inches of thin string wetted by distilled water, and sometimes through a string of twelve times the thickness, only twelve inches in length, and soaked in dilute acid (298.). With the thick string the charge passed at once; with the thin string it occupied a sensible time, and with the thread it required two or three seconds before the electrometer fell entirely down. The current therefore must have varied extremely in intensity in these different cases, and yet the deflection of the needle was sensibly the same in all of them. If any difference occurred, it was that the thin

string and thread caused greatest deflection; and if there is any lateral transmission, as M. Colladon says, through the silk in the galvanometer coil, it ought to have been so, because then the intensity is lower and the lateral transmission less.

366. Hence it would appear that *if the same absolute quantity of electricity pass through the galvanometer, whatever may be its intensity, the deflecting force upon the magnetic needle is the same.*

367. The battery of fifteen jars was then charged by sixty revolutions of the machine, and discharged, as before, through the galvanometer. The deflection of the needle was now as nearly as possible to the eleventh division, but the graduation was not accurate enough for me to assert that the arc was exactly double the former arc; to the eye it appeared to be so. The probability is, that *the deflecting force of an electric current is directly proportional to the absolute quantity of electricity passed,* at whatever intensity that electricity may be*.

368. Dr. Ritchie has shown that in a case where the intensity of the electricity remained the same, the deflection of the magnetic needle was directly as the quantity of electricity passed through the galvanometer †. Mr. Harris has shown that the heating power of common electricity on metallic wires is the same for the same quantity of electricity whatever its intensity might have previously been ‡.

369. The next point was to obtain a *voltaic* arrangement producing an effect equal to that just described (367.). A platina and a zinc wire were passed through the same hole of a draw-plate, being then one eighteenth of an inch in diameter; these were fastened to a support, so that their lower ends projected, were parallel, and five sixteenths of an inch apart. The upper ends were well connected with the galvanometer wires. Some acid was diluted, and, after various preliminary experiments, that adopted as a standard which consisted of one drop strong sulphuric acid in four ounces distilled water.

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* The great and general value of the galvanometer, as an actual measure of the electricity passing through it, either continuously or interruptedly, must be evident from a consideration of these two conclusions. As constructed by Professor Ritchie with glass threads (see Philosophical Transactions, 1830, p. 218, and Quarterly Journal of Science, New Series, vol. i. p. 29.), it apparently seems to leave nothing unsupplied in its own department.

† Quarterly Journal of Science, New Series, vol. i. p. 33.

‡ Plymouth Transactions, page 22.

Finally, the time was noted which the needle required in swinging either from right to left or left to right: it was equal to seventeen beats of my watch, the latter giving one hundred and fifty in a minute. The object of these preparations was to arrange a voltaic apparatus, which, by immersion in a given acid for a given time, much less than that required by the needle to swing in one direction, should give equal deflection to the instrument with the discharge of ordinary electricity from the battery (363. 364.); and a new part of the zinc wire having been brought into position with the platina, the comparative experiments were made.

370. On plunging the zinc and platina wires five eighths of an inch deep into the acid, and retaining them there for eight beats of the watch, (after which they were quickly withdrawn,) the needle was deflected, and continued to advance in the same direction some time after the voltaic apparatus had been removed from the acid. It attained the five-and-a-half division, and then returned swinging an equal distance on the other side. This experiment was repeated many times, and always with the same result.

371. Hence, as an approximation, and judging from *magnetical force* only, at present (376.), it would appear that two wires, one of platina and one of zinc, each one eighteenth of an inch in diameter, placed five sixteenths of an inch apart, and immersed to the depth of five eighths of an inch in acid, consisting of one drop oil of vitriol and four ounces distilled water, at a temperature about 60°, and connected at the other extremities by a copper wire eighteen feet long and one eighteenth of an inch thick (being the wire of the galvanometer coils), yield as much electricity in eight beats of my watch, or in $\frac{8}{150}$ths of a minute, as the electrical battery charged by thirty turns of the large machine, in excellent order (363. 364.). Notwithstanding this apparently enormous disproportion, the results are perfectly in harmony with those effects which are known to be produced by variations in the intensity and quantity of the electric fluid.

372. In order to procure a reference to *chemical action*, the wires were now retained immersed in the acid to the depth of five eighths of an inch, and the needle, when stationary, observed; it stood, as nearly as the unassisted eye could decide, at $5\frac{1}{3}$ division. Hence a permanent deflection to that extent might be considered as indicating a constant voltaic current, which in eight

beats of my watch (369.) could supply as much electricity as the electrical battery charged by thirty turns of the machine.

373. The following arrangements and results are selected from many that were made and obtained relative to chemical action. A platina wire one twelfth of an inch in diameter, weighing two hundred and sixty grains, had the extremity rendered plane, so as to offer a definite surface equal to a circle of the same diameter as the wire; it was then connected in turn with the conductor of the machine, or with the voltaic apparatus (369.), so as always to form the positive pole, and at the same time retain a perpendicular position, that it might rest, with its whole weight, upon the test paper to be employed. The test paper itself was supported upon a platina spatula, connected either with the discharging train (292.), or with the negative wire of the voltaic apparatus, and it consisted of four thicknesses, moistened at all times to an equal degree in a standard solution of hydriodate of potassa (316.).

374. When the platina wire was connected with the prime conductor of the machine, and the spatula with the discharging train, ten turns of the machine had such decomposing power as to produce a pale round spot of iodine of the diameter of the wire; twenty turns made a much darker mark, and thirty turns made a dark brown spot penetrating to the second thickness of the paper. The difference in effect produced by two or three turns, more or less, could be distinguished with facility.

375. The wire and spatula were then connected with the voltaic apparatus (369.), the galvanometer being also included in the arrangement; and, a stronger acid having been prepared, consisting of nitric acid and water, the voltaic apparatus was immersed so far as to give a permanent deflection of the needle to the 5½ division (372.), the fourfold moistened paper intervening as before*. Then by shifting the end of the wire from place to place upon the test paper, the effect of the current for five, six, seven, or any number of the beats of the watch (369.) was observed, and compared with that of the machine. After alternating and repeating the experiments of comparison many times, it was constantly found that this standard current of voltaic electricity,

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* Of course the heightened power of the voltaic battery was necessary to compensate for the bad conductor now interposed.

continued for eight beats of the watch, was equal, in chemical effect, to thirty turns of the machine; twenty-eight revolutions of the machine were sensibly too few.

376. Hence it results that both in *magnetic deflection* (371.) and in *chemical force*, the current of electricity of the standard voltaic battery for eight beats of the watch was equal to that of the machine evolved by thirty revolutions.

377. It also follows that for this case of electro-chemical decomposition, and it is probable for all cases, that the *chemical power, like the magnetic force* (366.), *is in direct proportion to the absolute quantity of electricity* which passes.

378. Hence arises still further confirmation, if any were required, of the identity of common and voltaic electricity, and that the differences of intensity and quantity are quite sufficient to account for what were supposed to be their distinctive qualities.

379. The extension which the present investigations have enabled me to make of the facts and views constituting the theory of electro-chemical decomposition, will, with some other points of electrical doctrine, be almost immediately submitted to the Royal Society in another series of these Researches.

Royal Institution,
15*th Dec.* 1832.

NOTE.—I am anxious, and am permitted, to add to this paper a correction of an error which I have attributed to M. AMPERE in a former series of these EXPERIMENTAL RESEARCHES. In referring to his experiment on the induction of electrical currents (78.), I have called that a disc which I should have called a circle or a ring. M. AMPERE used a ring, or a very short cylinder made of a narrow plate of copper bent into a circle, and he tells me that by such an arrangement the motion is very readily obtained. I have not doubted that M. AMPERE obtained the motion he described; but merely mistook the kind of mobile conductor used, and so far I described his *experiment* erroneously.

In the same paragraph I have stated that M. AMPERE says the disc turned "to take a position of equilibrium exactly as the spiral itself would have turned had it been free to move;" and further on I have said that my results tended to invert the sense of the proposition "stated by M. AMPERE, *that a current of electricity tends to put the electricity of conductors near which it passes in motion in the same direction.*" M. AMPERE tells me in a letter which I have just received from him, that he carefully avoided, when describing the experiment, any reference to the direction of the induced current; and on looking at the passages he quotes to me, I find that to be the case. I have therefore done him injustice in the above statements, and am anxious to correct my error.

But that it may not be supposed I lightly wrote those passages, I will briefly refer to my reasons for understanding them in the sense I did. At first the experiment failed. When re-made successfully about a year afterwards, it was at Geneva in company with M. A. DE LA RIVE: the latter philosopher described the results *, and says that the plate of copper bent into a circle which was used as the mobile conductor "sometimes advanced between the two branches of the (horse-shoe) magnet, and sometimes was repelled, *according* to the direction of the current in the surrounding conductors."

I have been in the habit of referring to DEMONFERRAND'S *Manuel d'Electricité Dynamique*, as a book of authority in France; containing the general results and laws of this branch of science, up to the time of its publication, in a well arranged form. At p. 173, the author, when describing this experiment, says, "The mobile circle turns to take a position of equilibrium as a conductor would do in which the current moved in the *same direction* as in the spiral;" and in the same paragraph he adds, "It is therefore proved *that a current of electricity tends to put the electricity of conductors, near which it passes, in motion in the same direction.*" These are the words I quoted in my paper (78.).

Le Lycée of 1st of January 1832, No. 36, in an article written after the receipt of my first unfortunate letter to M. HACHETTE, and before my papers were printed, reasons upon the direction of the induced currents, and says, that there ought to be "an elementary current produced in the same direction as the corresponding portion of the producing current." A little further on it says, "therefore we ought to obtain currents, moving in the *same direction*, produced upon a metallic wire, either by a magnet or a current. M. AMPERE *was so thoroughly persuaded that such ought to be the direction of the currents by influence*, that he neglected to assure himself of it in his experiment at Geneva."

It was the precise statements in DEMONFERRAND'S Manuel, agreeing as they did with the expression in M. DE LA RIVE'S paper, (which, however, I now understand as only meaning that when the inducing current was changed, the motion of the mobile circle changed also,) and not in discordance with anything expressed by M. AMPERE himself where he speaks of the experiment, which made me conclude, when I wrote the paper, that what I wrote was really his avowed opinion; and when the Number of the Lycée referred to appeared, which was before my paper was printed, it could excite no suspicion that I was in error.

Hence the mistake into which I unwittingly fell. I am proud to correct it, and do full justice to the acuteness and accuracy which, as far as I can understand the subjects, M. AMPERE carries into all the branches of philosophy which he investigates.

Finally, my note to (79.) says that the Lycée, No. 36, "mistakes the erroneous results of MM. FRESNEL and AMPERE for true ones," &c. &c. In calling M. AMPERE'S results erroneous, I spoke of the results described in, and referred to by the Lycée itself; but *now* that the expression of the direction of the induced current is to be separated, the term *erroneous* ought no longer to be attached to them.

April 29, 1833. M. F.

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* Bibliotheque Universelle, xxi. p. 48.

V. *On the Relation which subsists between the Nervous and Muscular Systems in the more perfect Animals, and the Nature of the Influence by which it is maintained.* *By* A. P. W. Philip, *M.D. F.R.S. L. & E. &c.*

Read February 7, 1833.

In the last paper which I had the honour to present to the Society, and which appeared in the Philosophical Transactions for 1831, I endeavoured, by comparing the various facts relating to the circulation of the blood, and by such additional experiments as seemed to be required, to free the subject from the confusion in which it had been involved by the various and contradictory experiments and statements of writers, and to ascertain the source and nature of the powers on which the motion of the blood depends.

In the present paper I propose to consider in the same way, another subject of equal importance, intimately connected with the preceding, and which has, by the same means, been involved in equal, and, from its more complicated nature, apparently greater perplexity; namely, the relation which subsists between the nervous and muscular systems, and consequently, between the nervous system and organs of circulation; for I think it will be admitted, from the statement of facts made in the paper just referred to, that the power of the vessels, like that of the heart, is a muscular power, and that on the combined power of the heart and vessels, the motion of the blood, in the ordinary states of the circulation, wholly depends. Having considered this part of the subject, I shall endeavour to point out how far we can proceed in ascertaining the nature of the nervous influence, the means by which the relation between the nervous and muscular systems is maintained.

I need hardly observe that in experimenting on a subject so complicated as the living animal body, much practice as well as caution is necessary in order to guard against erroneous conclusions, and thus obtain for physiology the certainty which can alone entitle it to be regarded as a science. There is none of its branches which has not suffered from the inaccuracy of experimentalists, and, from the complicated nature of the subject, none which has suffered more in this way than that to which I am about to call the attention of the Society.

Throughout the long inquiry in which I have been engaged, I have always been more anxious to secure any ground I had gained by a careful repetition of the experiments, than to proceed to further attempts; and I may be allowed to state, that although my experiments have been repeated by many of the first physiologists, both of this country and the continent, they have not in any instance been found inaccurate. The rules I have always followed, and which, if I may presume so far, I would recommend to all engaged in such inquiries, are to make the experiments in the presence of competent witnesses, and to repeat them as long as any doubt remains on the mind of any individual present. Were these rules strictly observed, physiology would cease to be perplexed by the inaccuracies in which the inexperienced and precipitate are constantly involving it.

But this has not been the only cause of perplexity in the subject I am entering on. The evil, both here and in other branches of physiology, has, in a great degree, arisen from writers either having been too apt to enter upon their inquiries and detail their experiments without having made themselves acquainted with the state of our knowledge at the time, and without having sufficiently weighed what had been done by others, even as far as they were acquainted with it; or, when little had previously been done, having drawn their inferences from too partial a view of facts. Thus, much confusion and contradiction have often been introduced even where the immediate results of their experiments were accurately observed and recorded, and they have been prevented from so directing their labours as to conduce to a gradual and steady accumulation of knowledge.

As the early physiologists present to us the results of their own imaginings, without even pretending to be possessed of any facts which directly support them; the later and more rational inquirers have, from the causes just enumerated, been often betrayed into conclusions equally erroneous: and even in making important additions to our knowledge, they have frequently done what they could to render them useless and sometimes injurious to the cause of science, which inferences, from a partial view of facts, often are, to a greater degree than those which are evidently suppositions, because they assume the semblance of legitimate deductions.

Thus it was that M. Le Gallois taught that the power of the heart is independent of the brain, but derived from the spinal marrow, and that the powers of circulation in every part of the body depend on the corresponding part of that organ; inferences apparently supported by the facts he adduces, but wholly inconsistent with others which escaped him: and yet, at first view, so much the necessary results of his ingenious experiments, all of which were correct, that the Royal Academy of Sciences at Paris, after they had been repeated in the presence of the celebrated Humboldt and other eminent academicians appointed by the Academy to witness their repetition, adopted all his conclusions, and were for some time followed by the learned of other countries. Many similar instances might be adduced*.

In order that I may distinctly lay before the Society the nature of the first question to be considered in the following paper, it is necessary to observe that it appears from experiments detailed in papers which the Society did me the honour to publish in the Philosophical Transactions for 1815, and since republished in my Inquiry into the Laws of the Vital Functions, that the power both of the muscles of voluntary and involuntary motion is independent of the nervous system, yet in both equally capable of being influenced by it, the nervous influence being the constant stimulant in the functions of the former class of muscles, and an occasional stimulant in those of the latter, which in all their usual functions are excited by stimulants peculiar to themselves; but that these classes of muscles are influenced by it in very different ways, each of the muscles of voluntary motion being under the influence of no part of the brain and spinal marrow, but the particular part from which its nerves arise; while each of the muscles of involuntary motion is under that of every part of these organs, from the upper surface of the brain and cerebellum to the lowest portion of the spinal marrow.

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* It was thus that Haller inferred, from finding that the heart cannot, like a muscle of voluntary motion, be excited through its nerves, that this organ cannot be directly influenced by the brain and spinal marrow, an error which has more extensively influenced the practice of medicine than any other into which physiologists have led us. The fact on which this inference is founded depends not on the heart being placed beyond the direct influence of the brain and spinal marrow, but on the nature of the ganglionic nerves.

In subsequent papers, published in the Philosophical Transactions, and in the experimental inquiry just referred to, I endeavoured to trace the final cause of this arrangement; and found that the functions, with the exception of the circulation, to which the muscles of involuntary motion administer, namely, secretion and the other assimilating processes, require for their due performance the united influence of every part of the brain and spinal marrow; while the muscles of voluntary motion are concerned in no function but that of giving motion to the different parts of the body to which they are attached, and consequently have no direct influence on the functions of life.

From the whole of the experiments on which is founded the view I have thus been led to take of the functions of the nervous system, and its relation to the different classes of muscles, it would appear that the brain and spinal marrow are the only active parts of that system; the nerves, whether cerebral or ganglionic with their ganglions and plexuses, being only the means of conveying and combining the influence of the various parts of these organs.

But as these inferences are less direct than those arrived at in the foregoing publications, and as very different opinions have been maintained both by preceding and subsequent physiologists, it appears necessary to review this part of the subject with more care, particularly as our view of many of the phenomena, both of health and disease, must be essentially modified by it. This therefore is the first question I propose to consider.

The various parts concerned in the functions of the living animal may be divided into active and passive, those in which the power resides, and those which only obey that power, but which are equally essential to the function. Thus, the belly of a muscle is the active; the tendon, the passive part of the organ. In the production and application of the bile, synovia and other fluids prepared for the purposes of the animal economy, the gland is the active, the ducts, as far as the peculiar office of the organ is concerned, the passive part of that organ; and I know of no way of ascertaining which is the active and which the passive part of any organ, but by observing which obeys the other in the function of the part.

Thus it has been pretty generally admitted with respect to what may be called the cerebral part of the nervous system, that is, certain parts of the brain and spinal marrow, with the nerves immediately proceeding from them, that the former are the active, the latter the passive parts of these organs; because we find that the power of the nerves is always proportioned to the excitement of the brain and spinal marrow, as that of the tendon is proportioned to the excitement of the belly of the muscle. Even this position however has by some been controverted, and it has been maintained that the nerves themselves in part supply the influence on which their functions depend.

But in whatever manner this part of the question, which it will be necessary to consider more particularly, may be disposed of, there is certainly, from the more complicated structure of the parts, better reason for regarding the ganglionic nerves with their ganglions and plexuses, as active in the formation of nervous influence, than the simple nerve which connects the cerebral mass with the muscle which it excites; and it has consequently been the opinion of many physiologists that the ganglionic system is concerned in the production of this influence, and some have gone so far as to regard it as independent of the brain and spinal marrow, and therefore the only source of the power of its own nerves.

As soon as it was found that the organs supplied with ganglionic nerves obey every part of the brain and spinal marrow, it was necessary to abandon the latter opinion, and we could see a reason for the complicated structure of the ganglionic system, independently of its supplying any part of the nervous influence. As each of the vital organs is sensible to the action of every part of the brain and spinal marrow, some apparatus capable of combining the influence of all these parts is evidently necessary, and none apparently could be better fitted for the purpose than that system, which, both by its ganglions and plexuses, and the frequent anastomoses, if I may use that expression, of its nerves, seems even at first view intended to combine the power of the various parts from which it receives nerves; and when those proceeding from the ganglions and plexuses are found by direct experiment to convey the influence of all those parts, the inference appears almost unavoidable.

We here have a proof that the organs supplied by ganglionic nerves obey the influence of all parts of the brain and spinal marrow, and consequently that the ganglionic nerves combine and convey the influence of all those parts;

and it is contrary to what we observe of the simplicity of the operations of nature that there should be another source of that influence.

In another fact we find an additional objection to such a supposition, for it appears from many experiments related in the papers and inquiry above referred to, that exactly in proportion as we increase or impair the power of the brain and spinal marrow, the functions of the ganglionic nerves are increased or impaired; still pointing out the brain and spinal marrow as the active, and the nerves with their ganglions and plexuses as the passive parts of the system; and these observations come with the more weight because those who have maintained that the ganglions supply nervous influence, have not even pretended to support their opinion by any facts directly bearing on the point.

If however it also appears from direct experiment that the ganglions and plexuses are capable of influencing the power of the ganglionic nerves, independently of any change induced on the brain and spinal marrow, however improbable the fact may at first sight appear, we must admit that there is in the former organs an additional source of the power possessed by those nerves.

It has been found that the action of the heart is immediately influenced by agents, whether stimulants or sedatives affecting any considerable part either of the brain or spinal marrow *. Can its action, in like manner, be affected by agents making their impression on the ganglions and plexuses? For the purpose of determining this point, the following experiment was made, in which Mr. CUTLER, and Mr. FIELD the well-known veterinary surgeon, were so good as to assist me, Mr. FIELD performing the operative part.

The heart continues to obey the effect of agents acting through the nervous system for a certain time, after what we call death, that is, the removal of the sensorial powers †; and this time is much prolonged if the circulation be maintained by inflating the lungs at proper intervals; for in all modes of death, except where the nervous system is so powerfully and suddenly impressed as at once to destroy all the functions, the nervous as well as the muscular survive the removal of the sensorial powers ‡; and the newly dead is on several

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* Papers in the Philosophical Transactions for 1815, and Experimental Inquiry into the Laws of the Vital Functions.

† Philosophical Transactions for 1829, and Experimental Inquiry.

‡ Ibid.

accounts a better subject than the living animal, for such experiments as the following, although the result is still more satisfactory if the animal can be so prepared as to destroy the sensibility as far as the experiment is concerned, without so completely destroying it as to interrupt respiration*.

Mr. Field partially divided the spinal marrow near the head in an ass in such a manner as to destroy the sensibility, as far as the experiment was concerned, but not to interrupt the respiration, thus bringing the animal into the best possible state for the experiment. It lay as still and suffered as little during it, as an animal quite dead in the usual sense of the word, while the circulation was more perfect than it could be under any artificial inflation of the lungs. In another respect, the state of the animal was particularly favourable, for Mr. Field succeeded in exposing the semilunar ganglion and its plexuses with a very trifling loss of blood, not I believe four ounces. The heart was then found to pulsate sixteen times in ten seconds, as ascertained by the pulsation of the arteries in the neighbourhood of the ganglion. The ganglion and its plexuses were then irritated by the point of the scalpel, and at length cut across in various directions; but although the beats of the heart were repeatedly counted during these operations, they continued uniformly of the same frequency. Spirit of wine was then applied to the wounded ganglion and plexuses, but without the least change in the beats of the heart. A strong infusion of tobacco in water was now applied, but with the same result, the beatings of the heart being still sixteen in ten seconds; nor could any variation in the force of the beats be observed in any part of the experiment.

It appears from this experiment that we cannot influence the organs supplied by the ganglionic nerves by causes affecting the ganglions and plexuses, independently of the brain and spinal marrow; and the inferences from this and the preceding facts are unavoidable, that the former organs make only a part of the channel through which the influence of the latter is conveyed; and that the peculiar office of the ganglions and plexuses is to combine the influence of the nerves which terminate and are blended in them, and send off nerves endowed with their combined influence, in consequence of which the parts which receive the nerves proceeding from them, become subject to every part of the brain and spinal marrow.

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* It has been shown in the publications just referred to, that in the more perfect animals, respiration is as much a function of volition as the motion of a limb, and consequently ceases when the sensibility is wholly destroyed.

Such being the case with respect to the ganglions and plexuses, it is not likely that we shall find the nerves themselves, whether ganglionic or cerebral, capable of supplying any part of the influence they convey; but that nothing may be taken for granted, this also is a point which must be determined by an appeal to facts.

It is to be recollected that here, as in other cases, the onus probandi rests with the asserter. This would still be the case, although his position were less improbable than that, while there is an evident and acknowledged source of nervous influence, and that adequate to the production of the phenomena, another source of it should exist. Those who maintain such an opinion must adduce the proofs of it. Let us inquire to what they amount.

While the connexion of the nerves with the brain and spinal marrow exists, the nerves are capable of exciting the muscles, causing the evolution of caloric which supports animal temperature, forming the secreted fluids from the blood, and supporting the other processes of assimilation by which the structure of the various organs is maintained*; but as soon as this connexion is intercepted, all these functions begin to fail, and soon cease, nor do we possess a single fact to prove that there are any means in the nerve itself of maintaining or renewing any of them. By mechanical impulse the power which remains in a separated nerve of the cerebral class, for even this is not the case with respect to the ganglionic nerves†, may be directed to its extremities and made evident by the excitement of the muscle in which it terminates; but independently of such an impulse we have no means of exciting a nerve separated from the brain and spinal marrow, even during the short time it retains the influence it has received from those organs‡.

The very circumstance of the nerves being the means of conveying the influence of the brain and spinal marrow affords a presumption that they are not themselves the source of a similar influence. The former is evidently their peculiar function, and it is so improbable that they should perform another of so different a nature, that it would require the most unequivocal proof of such a fact to induce its belief. The power of the nerves is not only as far as we see derived from the brain and spinal marrow, and soon ceases and cannot be renewed when they are separated from these organs, but is, as I have already had occasion to observe, at all times proportioned to the degree of excitement in them; nor can an instance be adduced in which a cause of increased nervous power makes its impression on the nerves themselves. For its degree as well as existence, then, the power of the latter depends wholly on the former organs; and this observation applies as strictly to the ganglionic as to the cerebral nerves. The brain and spinal marrow therefore possess all the characteristics of the active, the nerves of the passive parts of the system.

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* Philosophical Transactions for 1829, and Experimental Inquiry.

† It is true that the heart has been excited by galvanism through the medium of its nerves; but they may here act merely as conductors.

‡ In the living animal a nerve cut off from direct communication with the brain and spinal marrow but otherwise uninjured, will, as Mr. Brodie has shown, long retain this power, as we should à priori have expected. It retains its healthy structure, and its communications with other nerves.

It may appear at first sight that the phenomena of what has been termed the sympathy of nerves oppose the preceding views. On a careful review of these phenomena, however, they will be found to afford them additional support. They are all such as depend on changes in the central parts of the nervous system, and in no degree on any influence of the nerves on each other in their progress. As I have nothing to add to the statements I have already published on this subject, for the facts on which the foregoing positions are founded I beg to refer to the 106th and 107th pages of the third edition of my Inquiry into the Laws of the Vital Functions.

Another opinion respecting the function of the nerves has been maintained, and lately by a writer of great respectability *, which deserves to be considered, because it claims the support of experiment, and if well founded must essentially affect our opinion of the nature of the nervous influence.

Dr. Henry appears to admit the independence of the muscular power, but thinks he has rendered it more than probable that the nervous influence, instead of being only one of many agents, is the only one capable of exciting the muscular fibre; and consequently that all others act through it, so that they are not in fact stimulants to that fibre, but to the nerves alone, through which they influence it.

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* Dr. William Charles Henry's Critical and Experimental Inquiry into the Relations subsisting between Nerve and Muscle, in the 110th Number of the Edinburgh Medical and Surgical Journal.

It is true that as mechanical impulse affecting a nerve of voluntary motion is capable, after its separation from the brain and spinal marrow, of exciting, through it, the muscle in which it terminates, and we cannot be assured that we have separated from the muscular fibre the whole of the nerves with which it is so intimately blended; if we were in possession of no other facts on the subject, we should be led to the inference, that the excitability of the muscular fibre can only be influenced through its nerves; but when instead of a mechanical, we employ a chemical agent, we find the result very different. We attempt in vain to influence a muscle through the nerve which terminates in it by the most powerful agent of this description, yet such an agent when applied to the muscular fibre itself excites it as readily as the mechanical agent, which is supposed only to affect it through the nerve.

Even if the power of the chemical agent be gradually increased until the structure of the part of the nerve to which it is applied is destroyed, not only the muscle in which it terminates, but even the other parts of the nerve itself remain wholly unaffected. The nerve has not even the power of communicating the change to its adjoining parts. This was proved by the experiments of FONTANA, and confirmed by those of Dr. HENRY related in the paper just referred to.

What reason then have we for supposing, when a chemical agent applied to the muscular fibre excites it, that it operates through the nerves which still adhere to it. Such an inference implies that nerves in their progress wholly change their nature, a supposition for which there is not only not a shadow of proof, but against which the most convincing proofs, which analogy can supply, present themselves.

Dr. HENRY indeed adduces in favour of the opinion of agents affecting the muscles only through the intervention of the nerves, a fact which is well ascertained, and which I amongst others have frequently witnessed, that the excitability of distant muscles may be impaired by chemical agents applied to the sentient extremities of the nerves; but here the brain and spinal marrow intervene between the nerves affected by the agent, and the muscles influenced. The effect of the agent is communicated through the nerves to these organs, and the debility of the muscles is the effect of the morbid impression made on them.

For the same reason a similar effect is produced on the muscles by what surgeons call concussion of the brain. A strong solution of opium or tobacco thrown into the cavity of the abdomen, or suddenly applied to any other extensive and highly sensible surface, has the same effect on the muscles as a blow on the head. It affects them in consequence of the brain and spinal marrow being influenced, and probably, where the cause is most powerful, having their finer mechanism deranged by the impression made on the sentient extremities of the nerves; so that we have here only an instance of a well-known fact, that certain affections of the brain and spinal marrow are capable of impairing the excitability of the muscles through the medium of the nerves, the only medium of course through which they can operate*.

How can we suppose, it has been said in support of the same opinion, that a muscle covered by a membrane of condensed cellular substance, or in other instances by the more complicated serous or mucous membrane, should be affected by a chemical agent applied to the opposite surface of such membranes, if not through the medium of its nerves?

If the agent does not pass through the membrane, and is thus immediately applied to the fibres of the muscle, by what other means can the effect be produced? We have just seen that the nerves are incapable of communicating the impression except where the brain and spinal marrow intervene. Besides, those who adopt this explanation forget that in many instances at least, I believe in all, the agent, independently of transudation through membranes, is as little in contact with the nerves as with the muscular fibres. The cuticle possesses neither nerves nor vessels, yet, through it, chemical agents influence the muscles; and are not such agents actually received through the cuticle and conveyed into the mass of blood?

It seems to be a general law of the animal system, that all membranes are pervaded by certain chemical agents. The air comes not in contact with the blood unless it be through the membranes of the lungs; yet who doubts that the changes effected in this organ are the consequence of transmission of an agent to or from the blood?

When a more stimulating blood produces a more powerful action of the heart and blood-vessels, can it arise from any other cause than the transmission of the more stimulating agent through the fine membrane which lines these cavities? Without such transmission it is no more in immediate contact with the nerves than with the muscular fibres; and if it were, we know that the impression it makes could not be conveyed through them. The membrane which lines the internal is more delicate than that which lines the external surface of the hollow muscles, and, as we might à priori expect, the agent pervades the former more readily than the latter. Hence it seems to be that a strong solution of opium thrown into the cavities of the heart, intestines, &c., immediately destroys their power; while applied to their external surface it makes no, or comparatively little, impression on it, the final cause of which it is not difficult to perceive. It is necessary that the muscular fibres of those cavities should be exposed to the stimulus of their contents, but their external surface only requires to be supported by the firmness of its membranes.

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* Philosophical Transactions for 1815, and Experimental Inquiry.

Must not the chemical agents which influence the nose, the mouth, the fauces, the bowels, the bladder, &c. pervade the fine membranes which line them before they can act either on their nerves or vessels?

There appear to be but two modes of impairing the excitability of the muscles independently of the immediate application of the agent to the muscular fibre itself; the one I have just had occasion to mention, the effect of powerful agents on the brain and spinal marrow, acting through them on the muscles; the other, the excessive excitement of their own function, too powerful and long-continued contractions.

The poisons which impair their excitability all affect them in one or both of these ways, except as far as they act by their immediate application to them. Opium, which was employed by Dr. Henry in the experiments just referred to, acts in both ways. But the exhaustion produced in the muscles of voluntary motion when an animal is killed by opium, although in part arising directly from its effects on the brain and spinal marrow, particularly when it has been suddenly applied to a very sensible and extensive surface, is, under other circumstances, chiefly the consequence of the violent spasms excited in them.

The spasms produced by an over-dose of opium always assume the form of opisthotonos, but they are still more subject to remissions than in the disease properly so called; and, as in that disease, they are, during the remissions, often readily renewed by the slightest causes. Even the touch of the finger, although the sensibility, as far as feeling is concerned, is wholly destroyed, is sufficient for this purpose; so that in making the experiment the spasms may be rendered more or less frequent according to the circumstances in which the animal is placed. On examining the state of the muscles of voluntary motion after death, as I have repeatedly done, in animals killed by opium, one being left undisturbed, while in another a constant succession of spasms had been maintained, their excitability in the former I have found little, in the latter greatly impaired. All will admit that the general spasms here arise from the state induced on the brain and spinal marrow by the opium, and not from any particular change in the nerves; any cause exciting by its action on the former organs, or in any other way, the same contractions, would, we know, produce the same exhaustion.

On reviewing all that has been said, it appears that we have no reason to suppose that the nervous influence excites the muscles on any other principle than that on which all other stimulants operate, which it also resembles in the circumstance of its excessive application acting as a sedative. I have been at much pains, in the last edition of my Inquiry into the Laws of the Vital Functions, to point out that all agents capable of influencing either the nervous or muscular system, and whether they make their first impression on the mind or body, act either as stimulant or sedative according to the degree in which they are applied, the stimulant effect always arising from the less, the sedative from the greater application of them, and different agents being better fitted to produce the one or other of these effects.

Thus with respect to the nervous influence, the more powerful, within certain limits, the action of the agent on the brain and spinal marrow, the greater is the stimulant effect on both classes of muscles; but if it be extreme, as when a severe blow is inflicted on the head, instead of exciting, it impairs their power, in the same way that they are powerfully excited by a moderate application of electricity, but deprived of all power by its excessive application.

The foregoing observations are strikingly illustrated by the facts which have been ascertained respecting the nature of the nervous influence.

As this influence has been found to perform its functions in the animal economy after it has been made to pass through a space not less than a quarter of an inch between the divided ends of a nerve, we must suppose either that, like magnetism or gravitation, it is capable of extending to a distance from the body in which it exists, or of passing through other conductors than the nerves. Were the former opinion correct, the influence of a nerve would extend in all directions, which we know not to be the fact, nor would the presence of the nerve be necessary to its functions, which we uniformly find to be the case. The conclusion then from the foregoing fact is unavoidable, that the nervous influence is capable of its functions after having passed through, and consequently existed in, other conductors than the nerves. It is therefore not peculiar to the nervous system, but capable of existing elsewhere, and consequently is not to be regarded as, strictly speaking, one of the vital powers of the animal body, but as an agent employed by them. On the other hand we find that voltaic electricity, applied under the same circumstances, is capable of all its functions, of exciting the muscles, of causing an evolution of caloric from arterial blood, of forming from the blood the various secreted fluids, and maintaining all the other processes of assimilation on which the healthy structure of every part depends *.

A vital power has no existence except in the particular mechanism to which it belongs, and its functions are of a nature which admits of the substitution of no other power. The muscular power has no existence but in the muscular fibre. The peculiar powers of the brain and spinal marrow are inseparable from these organs; and for none of these powers can any other power be substituted.

The nervous influence is therefore an inanimate agent, if this expression may be used; that is, one capable of existing in inanimate nature, and, consequently, independently of the mechanism to which in the animal economy it belongs, and the functions of which can be performed by an agent which we know to be of this description; and, as appears from what has just been said, the action of this influence on the muscles obeys the same laws as that of other inanimate agents capable of exciting them.

The results of the experiments, on which the foregoing positions rest, are stated in papers which the Society did me the honour to publish in the Philo-

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* Philosophical Transactions for 1817, 1822, 1827, 1828 and 1829; and Experimental Inquiry.

sophical Transactions just referred to, and detailed more at length in the third edition of my Inquiry into the Laws of the Vital Functions. These experiments were publicly repeated with the same results at the Royal Institution in the presence of many of the first physiologists of this country, Sir Humphry Davy, Mr. Andrew Knight, Mr. Brodie, &c.*, and also on a larger scale, a great variety of animals being employed, by M. Breschet and other physiologists at Paris†.

When the nature of the preceding positions,—for it will be admitted that the manner in which they have been ascertained precludes any doubt of their accuracy,—and the necessary inferences from them are carefully considered, it will be found difficult to deny that the nervous influence and voltaic electricity are powers of the same nature, although I have not been able to succeed in causing the former to affect the galvanoscope. It is found in like manner that the electricity of electric animals is incapable of affecting the electrometer ‡.

As the nervous influence is capable of existing independently of the organs in which it operates in the living animal, and therefore is not a power peculiar to these organs, it must either be a power sui generis yet capable of existing in textures of the most dissimilar description, or one of those powers employed by nature in her other operations. All will admit that the latter supposition is the most probable. The better we understand the operations of nature the more simple we find them. We see an endless variety of results from modifications of the same principle. Whether the most probable opinion is here the correct one, can only be ascertained by determining whether any one of the powers which operate in the production of other phenomena is capable of all the functions of the nervous influence. When it is found that such a power exists, all doubt must necessarily cease.

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* Journal of the Royal Institution, Nos. XXII. and XXIII. vol. xi. and xii.

† Recherches Expérimentales sur les fonctions du système nerveux. 1er Mémoire; "De l'Influence du système nerveux sur la Digestion Stomachale," par M. Breschet, D.M.P. Chef des Travaux Anatomiques de la Faculté de Médecine de Paris, etc. H. Milne Edwards, D.M.P. et Vavasseur, D.M.P. ("Mémoire lu à la Société Philomatique le 2 Août, 1823,") "Extrait des Archives Générales de Médecine, Août, 1823."

‡ A paper entitled, "An account of some experiments and observations on the Torpedo (*Raia Torpedo*, Linn.) by John Davy, M.D. F.R.S. Assistant Inspector of Army Hospitals," in the Philosophical Transactions for 1832. In page 262, Dr. Davy observes, "In accordance with Mr. Walsh and my brother, I have in no instance seen the Torpedo affect the common electrometer, or exhibit any the slightest indications of a power of attraction and repulsion in air."

I have, as far as I know, in the preceding paper laid before the Society, or referred to, all the circumstances which ought to influence our judgement respecting the relation which subsists between the nervous and muscular systems of the more perfect animals, and the nature of the influence by which it is maintained; and shall conclude by shortly recapitulating the more important results.

It appears from all the facts which have been stated or referred to,—

That the power of the muscles both of voluntary and involuntary motion is independent of the nervous system; but that both are subjected to its influence, this influence being the constant stimulant in the functions of the former, but only an occasional stimulant in those of the latter, which in their ordinary functions are excited by stimulants peculiar to themselves.

That to the muscles of voluntary motion it is supplied from those parts of the brain and spinal marrow, from which the nerve of the particular muscle takes its rise; to each of the muscles of involuntary motion, from every part both of the brain and spinal marrow.

That these organs are the only active parts of the nervous system, and that the cerebral and spinal nerves on the one hand, and the ganglionic nerves with their ganglions and plexuses on the other, are only the channels through which their influence is conveyed, the power of both systems of nerves being at all times proportioned to the excitement of the brain and spinal marrow, and soon ceasing, and not to be renewed when their influence is withdrawn, and being uninfluenced by causes acting independently of these organs on either set of the nerves themselves or on the ganglions and plexuses.

That the ganglionic system of nerves, with their ganglions and plexuses, is the means of combining the influence of every part of the brain and spinal marrow, and bestowing it on the muscles of involuntary motion, as well as on the various secreting and other assimilating organs, those muscles being subservient to the functions of these organs, which it appears, from direct experiment, require for their due performance the combined influence of every part of the brain and spinal marrow.

That the manner in which the nervous influence affects the muscular fibre is not essentially different from that in which it is affected by other stimulants and sedatives.

That this influence is not an agent peculiar to the nervous system, but capable of existing elsewhere, and consequently not a vital power properly so called; which further appears from an agent which operates in inanimate nature being capable of all its functions.

That the brain and spinal marrow therefore, so far from bestowing on the muscular fibre its power, only supply an inanimate agent which, like all other such agents capable of affecting it, acts on it either as a stimulant or sedative, according to the degree in which it is applied; and that the whole of the facts relating to this agent, prove its identity with voltaic electricity, which has been found experimentally to be capable of all its functions.

We cannot review the phenomena of the animal economy without being struck with the extent and variety of the functions of the nervous influence. We not only find the intercourse between the animal and the external world maintained by it, the heart and vessels subjected to its controul, and secretion and the other assimilating processes immediately dependent on it*, but that by its means the animal body is formed into a whole, every part of it being capable of influencing every other. Can we be surprised then that it more extensively than any of the other powers of that body influences its morbid states? In two treatises on the various effects of Indigestion and on the prevention of Organic Diseases, I have endeavoured to point out the share it has both in the production and in the extensive and varied effects of these derangements, and how much therefore their proper treatment depends on correct views of its agency.

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* So completely are the assimilating functions dependent on the nervous influence, that if the eighth pair of nerves be divided in the neck, and one of the divided ends folded back, in order to prevent any passage of nervous influence between them, the structure of the lungs will, in from fifteen to twenty hours, be so deranged, that in many places not even a vestige of their natural structure will remain. When, however, voltaic electricity is sent through the lungs as soon as their nerves are divided, and maintained in such power as supports a gentle twitching in the fore legs of the animal, the structure of the lungs is preserved, being found, in all respects, as perfect as if their nerves had remained uninjured.—Philosophical Transactions for 1822 and 1827, and Experimental Inquiry.

From all that has been said, we are unavoidably led to the conclusion that the same principle which operates so extensively in other parts of nature, no less extensively operates in our own frames. It is capable of acting in concert with the vital powers, properly so called, as well as with the other powers of inanimate nature, thus forming the link, if I may be allowed the expression, by which these powers, so different in their nature, are enabled to co-operate.

In an Appendix to the seventh edition of my Treatise on Indigestion, I have at some length considered that function of the nervous influence by which the animal body is formed into a whole, and the manner in which it so extensively influences the phenomena and treatment of diseases.

Errata in the following paper.

Page 80, line 7, *for* it *read* the exhaustion.

——— line 8, *for* it, (last word but one in the line,) *read* sleep.

VI. *On the Nature of Sleep.* *By* A. P. W. Philip, *M.D. F.R.S. L. & E., &c.*

Read March 7, 1833.

OF all sciences Physiology is most exposed to causes of inaccuracy. The subjects of experiment are here the most complicated, and the phenomena at once the most varied, and bearing the least resemblance to those we are accustomed to contemplate. Hence it is that the groundless theories of our predecessors have been succeeded by the erroneous inferences of modern times, and the student is bewildered by contradictory evidence, until the conclusion is often forced on him, that, with the exception of some of the great outlines which have been established by such evidence as cannot be questioned, the subject is from its nature too perplexed to admit of a clear and satisfactory exposition. It will readily be admitted by those who are accustomed to contemplate the works of nature, that such a result is less the fault of the subject than the mode of investigating it; for although physiology is not a demonstrative science, it is as open to observation and experiment as any other; but greater caution in our inferences is required, in proportion as the sources of error are more numerous and less easily detected.

If the attempt to free some parts of this science from the confusion in which it has been involved, should expose me to the charge of presumption, because it can only be done by judging the labours of others, I have at least the apology of the necessity of the task, and of not a short life, in a great degree devoted to the subject; for although it is not more than twenty years since I commenced the investigation, the progress of which the Society did me the honour to report, my experiments on subjects immediately connected with that inquiry were commenced within a couple of years after I had begun the study of medicine, and have with intervals been continued ever since.

The two last papers which I had the honour to present to the Society, on the circulation of the blood, and on a subject immediately connected with it, the relation which subsists between the nervous and muscular systems, were written with the view here stated. The present paper is written with the same view, and forms indeed only a continuation of the subjects of these papers. It is evident from the most cursory view of the phenomena of sleep, that these subjects must be imperfect without an inquiry into the nature of a state which so materially influences the relation of the nervous and muscular systems.

There is no question relating to the living animal which involves a more general view of its phenomena than the nature of sleep, and, probably for this reason, none respecting which opinions are more vague and unsatisfactory. I propose to review these various phenomena for the purpose of ascertaining the organs in which its immediate cause exists, the laws on which it depends, and the effects it has on those parts of the system which are not concerned in its production.

We can perceive no final cause of the alternation of watchfulness and sleep, but such as has its origin in the imperfection of our nature. The end of life is enjoyment, and as sleep, if we may not regard it as a positive evil, prevents uniformity in the accomplishment of this end, to say nothing of the occasional inconveniences which attend it, were we as well acquainted with the principles of the animal, as we are with those of the solar system, we should probably find that this defect is, in the nature of things, as unavoidable, as the recurrence of darkness and a degree of cold which benumbs, and of heat which overpowers our faculties.

We shall never perhaps be able to tell why certain organs are capable of constantly maintaining their functions, while others require intervals of repose; but it is not difficult to perceive the necessity of the former part of the arrangement, because the permanent functions are those on which the life of the animal immediately depends, the intervals of repose belonging to those alone which are the means of intercourse with the world that surrounds him, and which therefore have no direct tendency to destroy life.

In tracing the relation of the nervous and muscular systems in the last paper I presented to the Society, I had occasion to recall to their recollection the different relations which the muscles of voluntary and involuntary motion bear to the nervous system, and to point out that the two sets of nerves, which form the medium of connexion between the active parts of that system, and these classes of muscles, obey different laws; the one conveying the influence of only certain parts of the brain and spinal marrow, the other conveying and combining that of the whole of these organs*. The former, it is now to be observed, while they are associated, on the one hand, with the organs of sense and the muscles of voluntary motion, are associated, on the other, with those parts of the brain and spinal marrow on which the mental functions depend†; the latter, namely, the nerves which arise, as far as we can ascertain by experiment, from all parts of those organs, being associated, on the one hand, with all those parts, and on the other, with the muscles of involuntary motion and the organs on which life immediately depends.

Thus we find in the more perfect animals two systems in a great degree distinct from each other; the former may be termed the sensitive system, that by which they perceive and act, and consequently are connected with the external world; the latter the vital system, that by which their existence is maintained. To understand the nature of sleep, we must determine the properties peculiar to each of those systems which have relation to that state, and the manner in which each is capable of influencing the other.

When the reasoning powers are fatigued by continued attention, the feelings by the excitement of the passions, the eye by the exercise of sight, the ear by that of hearing, the muscles of voluntary motion by powerful and repeated contractions, &c. the organs of all these functions cease to be excited. In order again to excite them, either stronger stimulants must be employed, or they must be refreshed by repose, during which, the functions of life still continuing, their due degree of excitability is restored; and they thus again become sensible to the usual stimulants of life.

The operation of this law in the sensitive system may be observed under all degrees of excitement. We can perceive a very sensible effect from slighter degrees of exhaustion than that which produces sleep. After sleep there is a vigour which gradually declines till we sleep again; so that every degree of excitement is followed by its corresponding degree of exhaustion. This law of our frame is so prevalent that physiologists have generally regarded it as belonging to every part of the system; but any degree of excitement which produces weariness, must, by a certain continuance of it, produce inability. It is evident, therefore, that were the organs of life to obey this law, a total failure of their functions must soon ensue. The sensitive system is restored because the powers of life remain; but if these powers suffer a similar exhaustion, by what means can their restoration be effected? This consideration alone might have convinced physiologists that their excitement is regulated by other laws.

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* The 57th and following pages of this volume of the Transactions.

† It has been shown experimentally in my Inquiry into the Laws of the Vital Functions, that the spinal marrow partakes of the sensorial functions. This is very little the case in man, but to a great degree in some animals.

It is evident indeed that the circulation continues uninterruptedly; but this has been explained by supposing that the heart and vessels during the intervals of their contractions recover their excitability, the exhaustion of which, during each contraction, has been regarded as the cause of the relaxation which succeeds it.

This theory appeared to apply well to the heart because during the intervals of its contractions the stimulus which excites it is removed; but how does it apply to the vessels from which the stimulus is never removed, and which can support the motion of the blood, as has been ascertained by many experiments, independently of the heart*? An organ exhausted by the action of any stimulant will never recover its excitability under the operation of the same agent which has exhausted it. The retina will never recover under the same degree of light which has impaired its power, nor the nerve of the ear under the same degree of sound.

A very simple experiment, however, demonstrates that the theory is as erroneous with respect to the heart as the vessels. If in a newly dead animal a ligature be thrown around the arteries attached to the heart so that it continues gorged with blood, its contractions, although ineffectual, still continue to recur with the same regularity as before the ligature was applied. When salt is sprinkled on the muscles of the newly dead animal, the effect is not permanent contraction succeeded by permanent relaxation, but a constant succession of contractions and relaxations, notwithstanding the continued application of the same stimulant, till their power is exhausted.

An experiment suggested by Dr. **Wollaston**, and with which he used to amuse his friends, strikingly illustrates the interrupted nature of muscular contraction, even where it is as nearly permanent as the nature of the muscle in its healthy state admits of†. If the elbows be made to rest on a table, and the end of a finger of each hand be pressed steadily on that part of the ear which covers the external passage so as to press it down forcibly on the end of that passage, we hear a rapid succession of distinct concussions. This he ascribed to our thus being made sensible of the motion of the blood in the vessels. But did it proceed from this cause, the repetition of the concussions would correspond with the beats of the heart. That it arises from the rapid succession of the contractions of the muscles of the arm by the action of which the end of the finger is pressed against the ear, may be proved by making the experiment in the following manner. Let the arms rest on the table in such a way as to press by their weight on the fingers which stop the ears, care being taken that the stopping of the ears be left to the weight of the arms, and in no degree produced by the action of the muscles. When we succeed in this attempt, all sense of concussion immediately ceases. It will be found that just in proportion as we succeed in preventing the action of the muscles, the noise abates, and when we perfectly succeed, ceases altogether. The same property of the muscle may be made perceptible to another of our senses. If a bird be allowed to rest on the finger, we perceive by the finger its weight alone. It so balances itself that the continued action of its muscles becomes unnecessary. But if the finger be moved, so that the bird is obliged to cling to it to maintain its place, we perceive a thrill which consists of the same rapid succession of concussions as in the former instance is perceived by the sense of hearing. The larger the bird is, they are of course the more distinct.

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* Experimental Inquiry, Part II.

† We have reason to believe that in spasm the muscle is in a state of permanent contraction, probably the cause of the pain which attends this state.

It is quite evident from all that has been said, that the state of the muscle is wholly different in the relaxation which intervenes between the contractions, from that which has supervened when the same stimulus can no longer excite it. Now it is not the first but the last of these states which indicates any loss of power in the muscle.

The whole phenomena of the animal body demonstrate that although it is true that a muscle may be exhausted by powerful and repeated contractions, it is not subject to the law which prevails in the sensitive system, that all degrees of excitement are followed by proportional exhaustion.

Thus it is that the muscles of voluntary motion often suffer exhaustion, because, being under dominion of the will, they are frequently exposed to excitement which is excessive either in its degree or duration, or both. Their exhaustion does not interfere with health, and for their restoration means are provided in the usual functions of the system. But the muscles employed in the vital functions obey a better regulated stimulant, which never, except in disease, produces any degree of excitement that impairs their power. In many diseases, we see the effect of such excitement. If it does not abate soon, and we cannot by artificial means in a short time reduce it, death is always the consequence: and even a short continuance of it produces a degree of debility that so impairs the powers of life as to render their restoration both slow and difficult. Thus it is evident that on the capability of the muscular fibre to be moderately excited, without suffering any degree of exhaustion, life immediately depends.

This property belongs equally to the muscles of voluntary as those of involuntary motion, the exemption of the latter from exhaustion in the healthy state of the system, not arising from any peculiarity in the nature of these muscles, but from the circumstances in which they are placed. In many diseases we find the muscles of voluntary motion in a state of excitement, that is, in a state of constant contraction and relaxation, which constitutes their state of excitement, during all our waking hours, that is, during all the time that those parts of the nervous system with which they are associated are capable of exciting them, without a sense of weariness or any other sign of exhaustion in them. The muscles of respiration which are, in the strictest sense, muscles of voluntary motion* are in a state of constantly renewed and gentle excitement during life. It is only in asthma and other cases, where their excessive action is required, that they experience any degree of exhaustion.

Thus the muscular fibre in its laws of excitement differs essentially from the other organs with which it is associated in the sensitive system. It is neither like them in the healthy state capable of uniform excitement, nor in it are all degrees of excitement followed by proportional exhaustion. But in the vital system, although all its other parts are capable of uniform excitement, the muscular fibre is not the only organ in which certain degrees of excitement are unattended by exhaustion. The same is true both of the ganglionic nerves and those organs of the brain and spinal marrow from which they derive their power, and which, it appears from direct experiment, are distributed throughout the whole extent of the brain and spinal marrow.

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* Philosophical Transactions for 1829, and Experimental Inquiry.

The secreting organs indeed, as well as those of circulation, are less vigorous in sleep than in our waking hours; but this, we shall find, besides that a diminished excitement cannot restore impaired excitability, but must, in proportion to its degree, still add to the exhaustion, is the necessary consequence of causes very different from their partaking of the exhaustion which prevails in the sensitive system. It is in disease alone that they suffer any degree of exhaustion, which in them produces a different species of debility, not an exhaustion analogous to that of the sensitive system; which it is even a means of preventing by impeding the functions of life, and thus indirectly proving a cause of irritation to this system.

It appears from all that has been said, that in the sensitive system alone we find organs capable of exhaustion from all degrees of excitement, and the exhaustion of which is consistent with a state of health, namely, the nerves of this system and those parts of the brain and spinal marrow with which they are associated; but it is a necessary inference from the facts stated in the last paper I had the honour to lay before the Society, that the former of these only obey the latter. To the latter alone therefore we must look for the exhaustion which is the immediate cause of sleep.

The parts of the brain and spinal marrow which are associated with the nerves and muscles of the sensitive system, gradually, from the effect of the usual stimulants of life, suffer such a degree of exhaustion that those stimulants can no longer excite them; and their functions, unless stronger stimulants be applied, necessarily cease. Impressions from external objects consequently are no longer perceived, and therefore cannot produce their usual effects either on mind or body. Thus the expenditure of excitability in those parts of the brain and spinal marrow, and consequently in the nerves and muscles whose functions depend on them, being arrested, the vital functions still continuing, such an accumulation of it takes place in all these organs as again renders them sensible to the usual stimulants of life, and the activity of the sensitive system is restored.

On the parts of the brain, and, in some animals, of the spinal marrow, as I have already had occasion to observe, which are associated with the nerves and muscles of the sensitive system, the mental functions depend. Hence the phenomena of dreaming, on which I shall make a few observations, immediately connected with the other parts of this paper, after considering the manner in which the vital, is influenced by the state of the sensitive, system in sleep.

We are now to consider the effects of sleep on those organs which have no share in its production.

One of the most important circumstances relating to the state of the sensitive system in sleep is that it is never so complete as, under all circumstances, to prevent its excitement. On this alone it depends, we shall find, that it has no fatal tendency. The degree of sensibility which remains in sleep is the distinguishing mark between it and the torpor of disease. That sleep alone is healthy from which we are easily roused. If our fatigue has been such as to render it more profound, it partakes of disease, that is, as will appear more clearly from what I shall have occasion to say of the different species of apoplexy, the vital system partakes of the debility, or some cause is operating which prevents the restoration of the sensitive system.

Distinct as the vital and sensitive systems are, we know that neither can long survive the other. In a paper which appeared in the Philosophical Transactions for 1829, I stated or referred to the facts which prove that in all modes of death, except the most sudden, arising from causes which so impress the nervous system as instantly to destroy all the functions, those of the sensitive system are the first which cease. The animal only dies when his means of enjoyment and intercourse with the world which surrounds him, no longer exist. This consequence is constant and never long delayed. It is necessary therefore to a clear view of the state of the functions of the animal body in sleep, to determine the bonds of union between the sensitive and vital systems, at first view so distinct, which render their existence, except for a very limited time, inseparable.

That the sensitive cannot exist independently of the vital system, is evident, on the slightest consideration; but the dependence of the latter on the former is much less so. The facts stated in the paper just referred to, prove that in the more perfect animals, the function of respiration, being the only vital function which requires the cooperation of the sensitive system, is here the bond of union. It appears from those facts that the muscles of respiration are, in the strictest sense, muscles of voluntary motion, the excitement of which consequently depends on the powers of that system. When the power of sensation wholly ceases, we cease to breathe.

So confused have been the ideas of physiologists on this part of the subject, that to account for the continued action of the muscles of respiration and their intimate connexion with the vital system, they have supposed a third class of muscles partaking of the nature of both the others, those of voluntary and involuntary motion, to which it has been alleged the muscles of respiration belong. If this be the case, these muscles must change their nature every instant, because they are the same muscles which are employed in a thousand other acts universally acknowledged to be mere acts of volition; and, on the other hand, when powerful causes impede the breathing, all the muscles of the trunk are employed in this function. Besides, the facts which have been laid before the Society prove not only that there is no such class of muscles as that here supposed, but that the laws of excitability are the same in all muscles, the difference between the muscles of voluntary and involuntary motion depending wholly on the nature of their functions and the circumstances in which they are placed. The nervous influence, although equally capable of influencing both, is supplied to them in different ways and for different purposes, the usual functions of the muscles of voluntary wholly, of involuntary motion in no degree, depending on that system. The action of the muscles of respiration continues during sleep, because the exhaustion of the sensitive system is not complete, and the cause which influences this system in their excitement, continues in our sleeping as well as waking hours; and the same is true of all other muscles of voluntary motion, as far as the causes which induce us to excite them are applied. In the soundest sleep we move our limbs if their posture be rendered uneasy. Are we not obliged to guard against these causes in sleep, else the motions they would produce would quickly rouse us. Those of respiration are too gentle to produce this effect.

The only change which takes place in the action of the muscles of respiration during sleep is, that in proportion as the sensibility is impaired they are excited less readily, and the act of respiration is thus rendered less frequent, a more powerful application of the cause being required; the consequence of which is, that when they are excited, the air is drawn in with greater force; hence, and from the relaxation which is apt to take place during sleep in the parts about the fauces, the cause of snoring*. Thus we generally observe that the snoring is the louder the slower the breathing, that is, the relaxation of the fauces being the same, the more profound the sleep. The loudest snoring I ever heard, so loud as to startle the attendants, was in the last ten minutes of the life of a person who died of a disease of the brain impairing the sensibility, and who only breathed three or four times during that space.

The other changes observed in the vital system in sleep are evidently the consequence of the diminished frequency of respiration. This necessarily produces a proportional diminution in the frequency of the pulse; the properties of the blood being less frequently renovated in the lungs, it less readily excites the heart and vessels, and the diminished force of circulation is as necessarily attended with a diminished formation of the secreted fluids. This state of the vital organs, in its turn, influences the sensitive system, and thus the sleep is rendered more profound. While health continues, however, the vital powers are never sufficiently impaired to prevent the perfect restoration of those functions by which the animal is again fitted for intercourse with the external world.

The foregoing positions are well illustrated by the symptoms of apoplexy, in which a cause exists that prevents this restoration, and which consequently point out to us in a more striking manner the influence of the sensitive on the vital system. Here we find that in proportion as the sensibility fails, the respiration, and with it the pulse, continue to become slower; and when it has failed altogether, so that no cause of irritation can excite any sensation, the respiration ceases, and the loss of circulation soon follows. In this way the patient dies in sanguineous apoplexy, where the cause of derangement is a gradually increasing pressure on the brain, in consequence of which its sensibility is at length extinguished. Here there is no original disease of the vital organs. Could the sensibility be sufficiently maintained to preserve a due frequency of respiration, and nourishment from time to time be introduced into the stomach, life would go on as in sleep, till the increasing affection of the brain, extending from the sensitive to the vital parts of that organ, so deranged the assimilating processes as to destroy life in this way.

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* Such facts are adduced in the paper last referred to as I believe will be admitted to prove that respiration is at all times an act of volition, excited by the sensation caused by the want of fresh air in the lungs; and the more the sensibility is impaired, the want must be allowed to become the greater, in order to excite the effort which relieves it.

The accumulation of phlegm in the lungs in apoplexy arises from these processes being deranged by the failure of nervous influence. I have repeatedly, in apoplexy, removed this accumulation of phlegm, the breathing becoming as free as in health, by causing voltaic electricity to pass through the lungs in the direction of their nerves. This, it is evident, can have no direct tendency to remove the disease, although by its means life may often be prolonged, and thus more time afforded for the application of the means of cure, this accumulation of phlegm greatly impeding the due change of the blood in the lungs, and thus conspiring with the diminished frequency of respiration to deprive it of its vital properties[*].

A short comparison of the symptoms of apoplexy from compression, with that which is with great propriety termed nervous, will throw additional light on this part of the subject.

It is shown by experiments detailed in papers which appeared in the Philosophical Transactions for 1815, that although the power of the heart and vessels is independent of the brain and spinal marrow, causes operating on these organs are capable of influencing them, and that even to the total destruction of their power. When, therefore, the cause of apoplexy, instead of being a gradually increasing pressure of the brain,—which I have found by experiment, however powerful it may be, has no direct influence on the action of the heart[†],—is of such a nature as, while it impairs the sensibility, also directly impairs the power of the heart and blood-vessels, we have a disease of a very different nature from apoplexy from mere compression. In the latter, if we can remove the cause of pressure and prevent its recurrence, we invariably cure the disease. There is no other cause of derangement. The vital functions are only impeded by the want of the due change of the blood in the lungs, in consequence of failure in the functions of respiration and assimilation. Death here is necessarily slow, because it always requires some time for the gradually increasing pressure either to destroy the sensibility, and consequently wholly stop respiration, or so derange the assimilating processes, as in this way to prove fatal, for from some peculiarity in the cause, the effect of which more readily than usual spreads to the vital parts of the brain, death, in apoplexy from compression, sometimes appears rather to arise from this derangement than the loss of sensibility, the phlegm gradually accumulating in the lungs till it wholly prevents the necessary change of the blood effected in them *.

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[*] Experimental Inquiry, third edition, Part III. On the application of the experiments to explain the nature and improve the treatment of diseases, Chap. 1.

[†] Ibid. Part II.

But when the cause, which impairs the sensibility, also through the ganglionic system immediately enfeebles the heart and blood-vessels, the course of the disease is very different. We have here a cause at once impairing the powers of circulation; and when it is excessive, death is often instantaneous. Such is the cause of death from blows on the head, which, when not sufficient to produce instant death, produce what is called concussion of the brain, in which a state analogous to syncope is combined with impaired sensibility. The circulation is doubly assailed by the direct diminution of the power of its organs, and a failure in the stimulating power of the blood, in consequence of its less perfect decarbonization, and the former being the more powerful cause, obscures the effects on the vital organs of the latter. The pulse, instead of being slow but regular, and of unimpaired strength, is feeble, irregular, and fluttering, and a general paleness of the surface indicates a degree of failure of circulation, far beyond what is observed in cases of mere compression.

All sudden and excessive affections of the brain may produce the same effects as the blow on the head. Thus, people have instantly expired from rage or excessive joy, and thus in the mobs of Lord George Gordon, some from the sudden effect on the brain through the nerves of the stomach, expired on taking a draught of spirit of wine which they had mistaken for common gin.

But it is not necessary that the cause, as in these cases, should be either sudden or violent to produce this species of apoplexy. A long-continued recurrence of slighter causes weakening the powers of the brain, often, along with them, gradually impairs those of the heart and blood-vessels, in the same way that an infusion of tobacco, applied to the brain in the experiments above referred to, impaired their powers. These are the most common causes of

* This accumulation of phlegm in the lungs has been found experimentally to be the uniform consequence of lessening the supply of nervous influence in the lungs.—Philosophical Transactions for 1827 and 1828; and Experimental Inquiry, Part II.

nervous apoplexy; and in proportion as their operation has been slow, the course of the disease is less rapid.

Thus we see it supervene in those who have been long exposed to the irritations which attend the more serious and confirmed cases of indigestion or long-continued causes of anxiety, particularly in gouty habits, in which there is often a great tendency to debility in the vital organs; and we readily perceive from what has been said, why apoplexy from such causes is so generally fatal. The powers both of the nervous and circulating systems are undermined, and with them the secreting and other assimilating processes which depend on them. The powers which ought to respond to our remedies have failed. Our efforts therefore are for the most part equally unavailing in restoring either the sensibility or the powers of circulation, and both are necessary to recovery.

FROM a review of the whole of the facts which have been laid before the Society, it appears,—

That in the brain and spinal marrow alone reside the active parts of the nervous system.

That the law of excitement in the parts of these organs, which are associated with the nerves of sensation and voluntary motion, is uniform excitement followed by proportional exhaustion, which, when it takes place to such a degree as to suspend their usual functions, constitutes sleep; all degrees of exhaustion which do not extend beyond them and the parts associated with them, being consistent with health.

That the law of excitement in those parts of the brain and spinal marrow which are associated with the vital nerves is also uniform excitement, but which is only, when excessive, followed by any degree of exhaustion, no degree of which is consistent with health.

That the vital, in no degree partaking of the exhaustion of the sensitive system in sleep, only appears to do so in consequence of the influence of the latter on the function of respiration, the only vital function in which these systems cooperate.

That the law of excitement of the muscular fibre, with which both the vital and sensitive parts of the brain and spinal marrow are associated, is interrupted excitement, which, like the excitement of the vital parts of these organs, is only, when excessive, followed by any degree of exhaustion. And

That the nature of the muscular fibre is everywhere the same, the apparent differences in the nature of the muscles of voluntary and involuntary motion depending on the differences of their functions, and of the circumstances in which they are placed.

I shall conclude this paper with a few observations on dreaming, immediately connected with the preceding parts of the subject.

Had we, independently of experience, been made acquainted with the nature of sleep, we might have foretold that dreaming,—pretty much as we find it,—would be its consequence.

We here find the sensitive parts of the brain, to which the powers of mind belong, and the parts associated with them, in a state of exhaustion, but not such exhaustion as prevents their being excited by slight causes, while other parts of the system are still in a state of activity. But it is only in the most perfect state of health, and such as we rarely enjoy, that the vital functions are performed without slight causes of irritation arising in some of their various and complicated processes, which tend to disturb the repose of the sensitive parts of the brain. Thus it is that indigestion and other internal causes of irritation produce dreaming. Such causes act partially, and therefore only partially excite those parts.

It seems greatly to influence the phenomena of dreaming, that in order to favour the occurrence of sleep, and thus as far as we can prevent unnecessary exhaustion, means are always employed at its accustomed times, to prevent, as much as possible, the excitement of the external organs of sense, and consequently those parts of the brain corresponding with them. This renders us the more sensible to causes of excitement existing within our own bodies, while, by the inactivity of those parts of the brain which correspond to the organs of sense, we are deprived of the usual control over such parts of the mental functions as are thus excited; the effect of which is greatly increased by the rapidity of the operations of the memory and imagination, when not restrained by some of the various means employed for that purpose in our waking hours. These are often objects of the senses, as written language, diagrams, sounds, and sometimes even objects of touch; but the most common is the mere use of words, independently of any object presented to our senses.

Any one may easily perceive how difficult it is to pursue a train of reasoning without this means of detaining his ideas for the purpose of steadily considering them and comparing them together. Now, in sleep, in consequence of the excitement of the brain being so partial, we are deprived of all these means; and our ideas pass with such rapidity as precludes all consideration and comparison. Our conceptions therefore are uncorrected by experience, and we are not at all surprised at the greatest incongruities. Why should we be surprised at our moving through the air, when we are not aware that we have not always done so? The mind of the dreamer differs from that of the infant in having a fund of ideas laid up in it which may by various circumstances be partially recalled; but it resembles it in being in other respects void of the results of experience, and consequently, with the exception of this partial operation of experience, of the means of correcting the ideas excited in it. In general, there is neither time nor means for doubt or hesitation.

Such is the rapidity of our thoughts in dreaming, that it is not uncommon for a dream, excited by the noise that awakes us, and which therefore must take place in the act of awaking, to occupy, when put into words, more than fifty times the space in the relation. It is a good illustration of what is here said, that when we dream that we are conversing, and thus obliged to employ words, the usual incongruities of dreaming do not occur. The ideas are sufficiently detained to enable us to correct the suggestions of the imagination. No man ever dreamt that he was telling another that he had been flying through the air.

Thus the peculiarities of dreaming arise from the partial operation of the causes of disturbance, and some of the sensitive parts of the brain being capable of excitement without disturbing others; and thus it is that the more near we are to awaking, the more rational our dreams become, all parts of the brain beginning to partake of the excitement; which has given rise to the adage, that morning dreams are true.

VII. *On the Existence of Four Distinct Hearts, having regular pulsations, connected with the Lymphatic System, in certain Amphibious Animals. By* JOHN MÜLLER, *M.D. Professor of Physiology in the University of Bonn. Communicated by* LEONARD HORNER, *Esq. F.R.S.*

Read Feb. 14, 1833.

I HAD long observed that it was very easy to obtain lymph from frogs, because these animals have large lymph-spaces (Lymphräume) immediately under the skin, and between the muscles. If the skin of the thigh of a frog be cut, the lymph will flow pretty freely, and may be collected in a watch-glass: it will continue fluid for ten minutes, and then coagulates; it is at first as clear as water. By this means, lymph can be exhibited to students, a matter of some importance, for medical men have very rarely an opportunity of seeing it, in the whole course of their lives; and the word has been very inappropriately applied to many heterogeneous things. Whether all the hollow spaces observed under the skin of the frog be true lymph-spaces, appears to me to be doubtful; several of them, however, most decidedly are so, especially those of the thigh.

None of the other amphibia have so large lymph-spaces as the frog has, but they all appear to be provided with remarkable *pulsating organs*, which direct the motion of the lymph. These I first observed in the frog.

1. *Posterior Lymphatic Hearts of Amphibia.*

These are most easily found in the frog, but they exist also in the toad, the salamander, and the green lizard; probably in all amphibia, the naked as well as those provided with scales. The organ is double, and, in the frog, lies on each side, behind the articulation of the os femoris, next the anus, in the regio ischiadica. Its regular contractions may be seen even through the skin, but more distinctly when that over the part is removed. The organ lies immediately under the skin. The arteria and vena ischiadica, the largest vessels in the thigh, run

immediately underneath the organ, but the motion of the blood in those vessels has no influence upon it. The contractions are neither synchronous with the motions of the heart, nor with those of the lungs, and are peculiar to the organ; for they continue after the removal of the heart, and even after the dismemberment of the animal. The pulsations of the two organs, on the right and left sides, do not keep time together, but sometimes alternate at irregular intervals. The organ is about two lines long, in the direction of the length of the animal, and about one line in breadth. When it contracts, it assumes an appearance as if it were separated into distinct compartments, and its internal surface has a spongy cellular structure. The fluid which moves in it is certainly not blood, but clear colourless lymph. If an incision be made into the organ, and air be blown into it, in the direction of the lower extremity of the animal, all the connected lymph-spaces of the thigh and leg are filled with air. These lymph-spaces in the thigh, lie partly under the skin, and partly between the muscles, and coming from the anterior and posterior sides of the thigh, they unite in several large lymphatic trunks behind the organ, and through these trunks the organ itself can be inflated. The inflation of these trunks, or of the organ, fills with air a large lymph-space lying under the skin, at the posterior external part of the abdomen, and another of the same kind between the abdominal muscles and the peritoneum, in the same situation on each side. Sometimes also a large lymphatic vessel, having a very thin coat, becomes filled; this leads, in an upward direction, to the arteria iliaca, appears to come in contact with that on the other side, and ascends further towards the aorta abdominalis, like the ductus thoracicus; but this vessel cannot be further inflated in an upward direction, and it is possible that the lymph, from the posterior part of the abdomen, goes to the posterior lymphatic heart, while the lymph from the intestinal canal and anterior portion of the abdomen goes to the anterior lymphatic heart. If the lymphatic heart be inflated, in the direction of the upper extremities of the animal, a superficial lymphatic vessel becomes filled, which proceeds from the back into the organ. At the entrance of all these lymphatic vessels there appear to be valves, so that distention does not always take place when the organ is inflated. With this exception, the air passes freely from the lymphatic spaces of one side of the animal into those of the other. Neither the lymphatic spaces nor the lymphatic vessels exhibit the least trace of action in themselves; it is the lymphatic heart alone which pulsates.

The connexion of the organ with the venous trunk of the thigh, on the same side, is very remarkable; for it appears clearly to conduct the lymph of the hinder extremities, as well as that of the posterior part of the abdomen and of the back, into this venous trunk.

I must here make some observations upon the posterior part of the venous system of the frog. The veins of the hinder extremities are, the vena cruralis and the vena ischiadica, and these unite above the thigh by a large transverse anastomosis. The vena iliaca is the continuation of the vena ischiadica, then becomes the vena renalis advehens Jacobsonii, which passes into the kidneys after receiving branches from the posterior region of the abdomen. The transverse anastomosis of the vena cruralis and ischiadica passes, in the regio pubis, into the vena abdominalis anterior impar, so that both cross veins unite in a semicircle, from the middle convexity of which the vena abdominalis anterior springs, while the extremities of the semicircle pass behind, into the venæ ischiadicæ. The vena abdominalis anterior receives the blood of the abdominal muscles, and, as happens in all amphibia, passes to the vena portæ of the liver. Thus the blood of the posterior part of the body does not immediately reach the vena cava inferior, but, according to JACOBSON, first passes through the vena advehens of the liver, and the venæ advehentes of the kidneys*.

The venæ advehentes of the kidneys, the vena abdominalis anterior cum venâ portæ become filled with air each time the lymphatic heart of the regio ischiadica is inflated, while the air passes into the vena ischiadica which lies under the lymphatic heart, through a venous branch, and then passes further, partly into the vena renalis advehens of this side, partly through the venous semicircle into the vena renalis advehens of the other side, and into the vena abdominalis anterior. This connexion of the lymphatic heart with a branch of the vena ischiadica is seen very perceptibly whenever an incision is made into the organ, and it is injected with quicksilver by means of a steel syringe. In that case, all the above-named veins become filled with quicksilver.

The posterior lymphatic hearts are somewhat difficult to find in the salamander and lizard, because their skin adheres very firmly; but in these animals they also lie under the skin, at the root of the tail, in a lateral position, behind the os ilium.

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* For an account of the venous system of amphibia, see JACOBSON in MECKEL's Archiv für Physiologie, 3 band, 1817, p. 147.

2. *Anterior Lymphatic Hearts of Amphibia.*

In a paper inserted in POGGENDORF's Annalen der Physik, 1832*, I alluded to the discovery I had made of the *posterior* lymphatic hearts; the *anterior* lymphatic hearts I only lately discovered. I read in an English medical journal that Dr. MARSHALL HALL, the author of a valuable work entitled "Essay on the Circulation of the Blood, London, 1831," had observed an artery in the frog, which continued to beat after the removal of the heart. It immediately occurred to me that this might also be the indication of the existence of a lymphatic heart; I procured the original work, which was only known in Germany by a short extract from it in FRORIEP's Notizen, and I then saw the figure of the injected artery in that meritorious book. Dr. HALL's observations are entitled to much respect, but he has, in this case, been deceived; and the proofs which he conceived he had thereby obtained of the muscular contractility of the arteries, rest on no better foundation than others which have been brought forward. The pulsating vessel in that place is a vein, and indeed a branch of the vena jugularis; and its pulsations are produced by a lymphatic heart which is connected with the vessel, and sends lymph into it. If the lymphatic heart be cut, the pulsation of the vein in question instantly ceases.

The anterior lymphatic hearts lie on each side, upon the great processus transversus of the third vertebra. The organ is found at once if the scapula be carefully raised and partly cut away. It lies immediately under the posterior end of the scapula, and partly projects beyond the posterior edge of the processus transversus of the third vertebra, so that it can be seen from below, although not so conveniently. It is of a round shape, and anteriorly somewhat pointed where it is connected with the vein in question; it is almost as large as the posterior lymphatic heart. The fluid which is sent from it into this vein is colourless; the transparency of the organ shows very clearly that it is so. This vein receives at the same time its blood from fine venous branches, which run above and beneath the organ, and even pass over it. At the moment of contraction of the lymphatic heart, the vein is the most distended, because it then receives the lymph; but when the lymphatic heart is relaxed, the vein collapses, and becomes more slender. If the lymphatic heart be injured, no further alteration takes place in the diameter of the vein. The lymphatic heart receives the lymph of the anterior part of the body, and probably also that of the intestinal canal, in order to send it into this vein. If an incision be made into the organ, and if it be inflated, immediately lymphatic spaces in the axilla become filled with air. By blowing in a direction towards the head, this vein, which I have said receives lymph from the organ, is filled with air; and if the organ be injected in the same direction with quicksilver, by means of a steel syringe, this vein, and through it the vena jugularis, as far as the vena cava superior, will be filled with quicksilver. The vein which proceeds from the organ forwards, in a direction parallel to the columna vertebralis, is, like most other veins, of a blackish colour, and unites with a vein of the occiput. The small trunk produced by this union, viz. the vena jugularis, now descends, receives venous branches from the scapula and from the axilla, and finally receives a vein of the region of the throat, and then ends in the vena cava superior, at the place where a great vein of the arm also enters the vena cava superior. This vein of the arm conducts a great part of the blood of the arm back, and receives a large vein of the skin of the trunk. The vena cutanea is remarkable on this account, that the blood of the skin of the abdomen arrives almost through it alone at the vena cava superior, while the blood of the abdominal muscles passes through the vena abdominalis anterior to the vena portæ, and from the posterior part of the abdomen into the venæ renales advehentes. The branches of the vena cutanea which go to the vein of the arm, come from the inferior and lateral part of the skin of the abdomen, and from the back. Smaller branches of the same vein unite behind with veins which go to the venous system of the thigh.

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* Beobachtungen zur Analyse der Lymphe des Blutes und des Chylus.

I have never discovered a trace of motion in the cysterna chyli and ductus thoracicus of mammalia.

In the vascular system of the blood, there certainly are particular places, besides the heart, which are capable of contractions; as for example, the bulbus aortæ in fishes and batrachia, the venæ cavæ where they enter the atrium, and the pulsating organ discovered by Dr. Marshall Hall at the end of the vena caudalis of the eel, where that organ receives the venous branches of the extremity of the tail, and conducts its blood into the vena caudalis. But organs of pulsation in the lymphatic system have hitherto been altogether unknown; it is not probable that they should exist only in amphibia, and important discoveries of a like nature in the higher animals, such as birds and mammalia, may be expected;—my researches, as regards these, have however been hitherto unsuccessful.

VIII. THE BAKERIAN LECTURE.—*Experimental Determination of the Laws of Magneto-electric Induction in different masses of the same Metal, and of its Intensity in different Metals.*

By S. HUNTER CHRISTIE, *Esq. M.A. F.R.S. M.C.P.S. Soc. Philom. Paris. Corresp. &c.*

Read Feb. 28, 1833.

MR. FARADAY'S highly interesting papers, entitled "Experimental Researches in Electricity," having been referred to me, to report on, by the President and Council of this Society, I necessarily entered minutely into all the experiments and conclusions of the author, and the more so that I had had the advantage of witnessing many of the most important of these experiments. It is foreign to my present purpose to descant upon the value of Mr. FARADAY's discovery, or the merits of his communication; the President and Council have marked their opinion of these by the award of the Copley Medal: but I may be permitted to state, that no one can concur more cordially than I do in the propriety of that award.

Agreeing as I did generally with the author, both in the views which he took of the subject, and in the conclusions which he drew from his experiments, there was one, however, which I felt great difficulty in adopting, viz. "That when metals of different kinds are equally subject, in every circumstance, to magneto-electric induction, they exhibit exactly equal powers with respect to the currents which either are formed, or tend to form, in them:" and that "the same is probably the case in all other substances." Although the experiments might appear to indicate that this was possibly the case, I did not consider them to be conclusive. The most conclusive experiment, that of two spirals, one of copper and the other of iron, transmitting opposite currents, was quite consistent with the absolute equality of the currents excited in copper and iron; but, at the same time, the apparent equality of the currents might be due to their inequality being counteracted by a corresponding inequality in the facility of transmission.

In order to determine whether the electric currents* excited in all metals were, under the same circumstances, of equal intensity, I proposed to subject different metals directly to the same degree of magneto-electric excitation, in such a manner that the currents excited in them should be in opposite directions, and that these opposing currents should have the same facility of transmission, by which means the difference of their intensities might be measured.

With this view a helix of copper wire covered with silk, of sixty-five coils, was formed on a cylinder about an inch in diameter, so that, when vertical, the coils turned from below upwards in the direction of the sun's daily motion. Another helix of iron wire, of exactly the same gauge as the copper, covered with cotton, and likewise of sixty-five coils, was formed on the same cylinder, but with the coils turned in the contrary direction to those of the copper wire helix. The coils of the one helix were introduced between those of the other, so that they crossed each other alternately, and then a cylinder of soft iron covered with paper was introduced within the two. The wire at the ends of each helix was uncovered and brightened for about two inches, and the corresponding ends were twisted together in good metallic contact: and to these were united the wires of the galvanometer, as in Plate III. fig. 1., where M is the marked pole of the magnet with which the iron cylinder A′ B′ was brought into contact, and U its unmarked pole; A the wire leading out from the galvanometer above the needle, and B that below it.

The galvanometer consists of sixty-six nearly rectangular coils of the same copper wire, covered with silk, as that forming the helix, to which is adapted a compound needle consisting of two very light needles of fine wire, two inches and three quarters long, connected by their centres, by the quill of a small feather, so that their contrary poles are vertically over each other when suspended. This compound needle is suspended by a single fibre of silk. The lower needle traverses freely within the coils of the wire, and the other above

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* I adopt the term electric current, or current of electricity, as a convenient mode of expression; and by it, I would be understood merely to imply that a conductor is in a peculiar state, manifested by certain effects produced on a magnetized needle; but I would not be considered as adopting any theoretical views on the subject:—whether this state consist in the *motion* of a peculiar fluid in the conductor, in the arrangement of one previously existing, or in the excitement of a power previously latent, I am not at present inquiring.

Fig. 1.

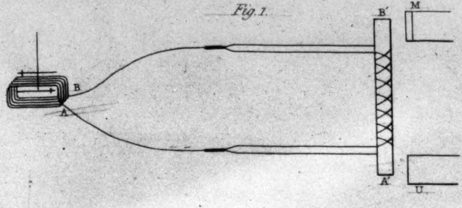

Fig. 2.

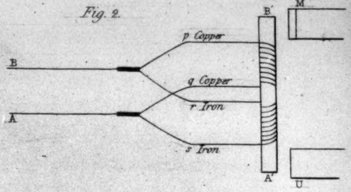
B′
M
p Copper
B
q Copper
r Iron
A
s Iron
A′
U

Fig. 3.

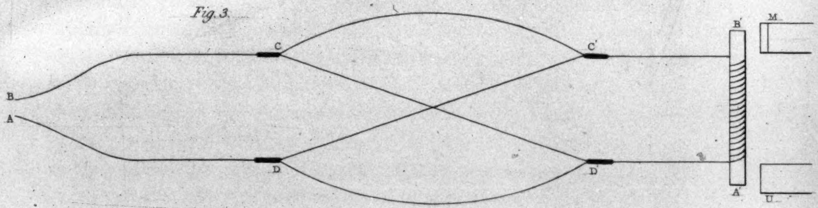
B′
M
C
C′
B
A
D
D′
A′
U

Fig. 4.

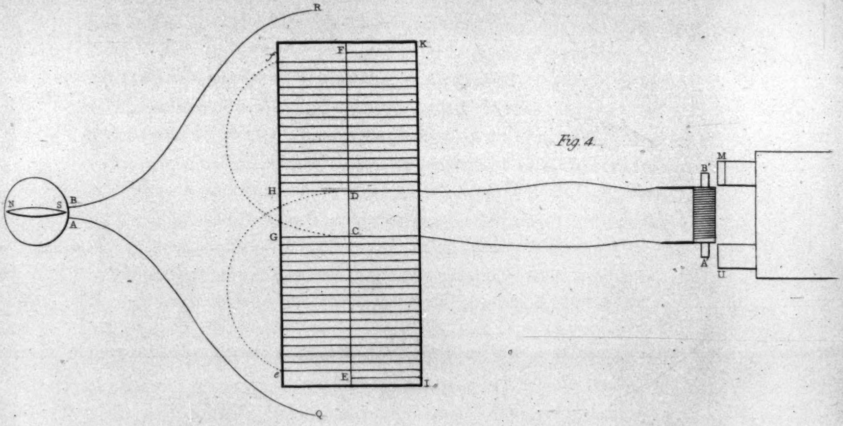
R
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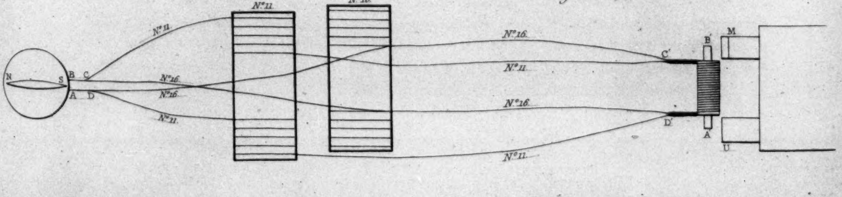
Fig. 5.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them; the intensity of the upper exceeding that of the lower just sufficiently to give it a small directive force in the meridian.

However small the difference of the currents excited in the two metals might be, I had no doubt I should detect it by means of the large magnet belonging to the Royal Society, which had been entrusted to me; but I did not expect that the difference would be of this minute character, and the results which I obtained, on making the experiment, fully justified the views with which it was undertaken. The direction of the coils of the helix of copper wire was such that, according to Mr. Faraday's experiments, if this alone had been round the iron cylinder, on its contact with the magnet being made, so that the end B′ should be applied to M, and A′ to U, the unmarked end of the upper needle would have moved west, and on the contact being broken it would have moved east. The coils of the helix of iron wire being in the opposite direction, precisely the reverse of this would have been the case had this helix alone been round the iron cylinder. I have here referred the direction of the galvanometer needle's motion to that of the unmarked or south end of the upper needle, and the same must be understood in all the observations which follow *.

On the contact of the iron cylinder with the magnet being made, B′ with M and A′ with U, the needle moved west, and on the contact being broken, it moved east; the needle in each case spinning round several times. Turning the cylinder with the helices end for end, so that B′ was opposite to U, and A′ to M; the contact being made, the needle moved east; and being broken, it moved west, in each case, as before, several times round.

The ends of the copper helix being separated from the galvanometer wire, so that the ends of the iron helix were alone connected with that wire, on contact being made, B′ with M, and A′ with U, the needle moved east; and on its being broken, west; but in neither case was the motion so rapid as it had been in the contrary direction, when the needle was only impelled by the excess of the force of the current from the copper helix above that from the iron helix.

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* In consequence of the position of my galvanometer, which was not one of choice, but almost of necessity, I could not accurately observe the motion of the marked or north end of the upper needle, and consequently always noted that of its south end; and I prefer giving the observations as they were set down, to the risk of making confusion, by omissions that might occur in altering east for west, and west for east, throughout the observations.

These experiments showed clearly that the force of the current from the copper helix exceeded considerably that from the iron one; and appeared even to indicate that the intensity of the current from the iron wire was less than half that from the copper. As however the coils of the two wires were necessarily loose round the cylinder, they could not be kept in exactly the same relative positions throughout their whole lengths, and consequently no very precise estimate could be formed of the relative intensities of the two currents. I therefore adopted a somewhat different arrangement.

Sixteen feet of the same copper wire was coiled as before on one end of an iron cylinder, about seven eighths of an inch in diameter, covered with paper; and sixteen feet of the iron wire was coiled in the contrary direction on the other end of the cylinder, there being seventy-one coils and a quarter of the copper wire, and seventy-two coils and a quarter of the iron wire. The ends of the wires being uncovered and made bright for good metallic contact, those ends of the copper and iron helices towards the same end of the bar were then united, and the galvanometer wires connected with them in good contact as in fig. 2. On contact of the iron cylinder with the magnet, B′ with M, A′ with U, being made, the needle moved west; and it moved east, on contact being broken; spinning round several times in each case.

The wires *q* and *r* were now separated from *s* and A, *p* and B, and then united together, so that the current excited in the copper must, in its transmission to the galvanometer, pass through the iron wire, and the current in the iron be transmitted through the copper wire. Contact being made, the first motion of the needle was east, then west; contact broken, the needle appeared to receive a sudden check upon its first motion, but it was difficult to say in which direction the first motion was, the motion being but just perceptible in any case, and the direction appearing to depend upon the manner in which the contact happened to be made. This form of the experiment is nearly that of Mr. Faraday's, to which I have already referred, and the results perfectly accord with those which he obtained. As the copper and iron helices were, as nearly as possible, subjected to the same inductive force, and that force was of the most powerful description, this experiment shows very clearly that however the intensity of the current excited in the copper helix might exceed that in the iron, this excess was very accurately counterbalanced by the greater difficulty of transmission offered by the iron wire to the stronger current of the copper wire, and the greater facility afforded by the copper wire to the transmission of the weaker current of the iron wire: or, in other words, that the intensities of the currents excited in the two wires were very accurately proportional to their conducting powers.

I had intended, by means of the arrangement fig. 2, to determine the relative intensities of the currents excited in different metals by reducing the number of coils of the stronger metal, until the current excited in it should be exactly equal to that in the weaker, which would be determined by no current being transmitted to the galvanometer; but a strong objection to this affording an accurate measure of the intensities was, that the several coils of the helices might not be subjected to the same intensity of magnetic action; and as at the same time that this objection occurred to me, a more accurate method of determining both the relative intensities of the currents excited in different metals under precisely the same circumstances, and their conducting power, presented itself, I prosecuted the inquiry no further in this form of the experiment, which, however, had fully answered the purposes for which it was originally intended. The arrangement which I proposed making possesses many advantages, not only for the purposes which I had in view in the first instance, but in a much more extensive field of inquiry. It affords a very accurate measure of the difference of the intensities of two electric currents, whether they arise from the same source, and are merely modified by circumstances, or have different sources; and whether the source be the magnet, the voltaic battery, or the common machine. And it affords likewise a very accurate measure of the conducting powers of different substances. The general nature of the arrangement, to which the term differential arrangement may with great propriety be applied, will be fully understood by the following experiment made with the same views as those which I have already described.

Four wires, each exactly four feet long, two being of copper and two of iron, of precisely the same diameter, having their ends brightened, were carefully twisted like the strands of a rope at their ends, two and two together, but crosswise, so that, at the corresponding ends, each of the copper wires being united with an iron wire, at the other end it was united with the other iron wire, as in fig. 3. The ends of the galvanometer wire were then brightened and laid carefully between the strands at C, D, and the ends of two copper wires being laid between the strands at C′, D′, the other ends of these were brought in contact with the ends of a compound copper wire helix wound round the iron cylinder B′ A′, and composed of eleven helices of about sixty-five coils each, united at the corresponding ends. Care was, in all cases, taken to prevent metallic contact everywhere but at the parts of the wires specified. On the contact of B′ with M, and A′ with U, the needle moved round east; and on the contact being broken, it moved round west. Reversing the iron cylinder with the helix, so that the contact now was of B′ with U, and A′ with M; on making this contact, the needle moved round west; and on breaking it, the needle moved round east. These results, it is almost unnecessary to say, agree with the preceding experiments.

On the contact of the ends of the iron cylinder with the poles of the magnet being made or broken, a current of a certain intensity being excited in the helix round the iron cylinder, became, at the points C′ D′, the source of currents in the copper and iron wires; at the points C D, equal facilities were afforded by the wires C B, D A, for the transmission of these opposing currents to the galvanometer, where, consequently, their difference might be very accurately measured. Or viewing the subject in a somewhat different light, at the points C′ D′, two routes are presented to the current excited in the wire of the helix, one through the copper wires, the other through the iron, and the effect at the galvanometer would measure the difference in the conducting powers of the two metals. According to the first view, the intensity of the current excited in the copper wire exceeded that in the iron wire; according to the second, the conducting power of the copper, in the same manner, exceeded that of the iron*.

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* As in a conversation which I had previously had with Mr. Faraday on this subject, I had stated to him the difficulty I felt respecting the conclusion drawn from his experiments, "Experimental Researches," 201, when I had obtained these results, I communicated them to him, and was immediately informed in reply that he had anticipated my corrections by further experiments of his own. It was satisfactory to find that we had thus, unknown to each other, and by independent experiments, arrived at the same conclusion. In a written Report to the President and Council on Mr. Faraday's paper, I had stated the difficulty I had felt and the conclusions I had arrived at, and it was decided that this Report should be read at one of the evening meetings of the Society, together with an account of the experiments to which I referred. By some mistake, the Report on Mr. Faraday's first series of expe-

Having in this manner determined that a considerable difference existed in the intensity of the currents excited under precisely the same circumstances in two metals, iron and copper; I then determined, in the same manner, the order of the relative intensities and conducting powers of several of the metals. As, however, in this inquiry some questions arose respecting the intensities of the currents excited in different lengths of the same wire, I shall first describe the experiments made with the view of determining the law of the intensity as depending upon the length and diameter of the wire through which the current is transmitted.

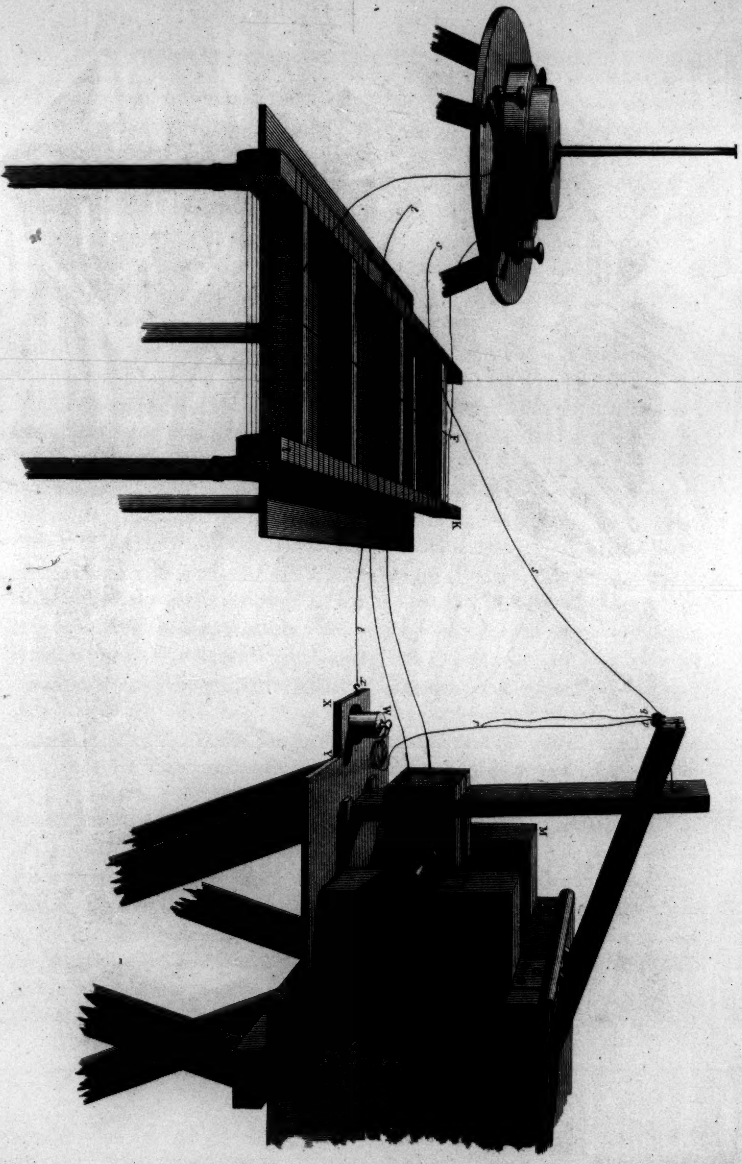
On the Law of Variation of Magneto-electric Intensity in different masses of the same metal.

I. *In different lengths of the same wire.*

According to the method of experimenting, which I have described, I could very readily obtain accurate measures of the difference in the intensities of the currents excited in or transmitted through two wires of different lengths, by crossing the wires as in fig. 3; but although it was an object to ascertain this difference, still it was necessary to have measures of the intensities themselves. I had found, in previous experiments, that considerable differences were observable in the effects at the galvanometer, according to the manner in which the contact of the iron cylinder with the magnet happened to be made or broken. This was not a matter of so much moment when only the differences of forces were to be determined, and was of none when the opposing forces counterbalanced each other; but it became of the greatest consequence when measures of the forces themselves were to be obtained. It was therefore necessary, in the present inquiry, that the contact should be made, and likewise

riments was read in its place; and therefore, when I was aware of this, I declined having the second Report either read or printed, as there had been already so much on the subject. I feel it necessary to state this, in order to account for the reading and printing of a report so long after it had been made, and at a time when it appeared quite uncalled for. I had intended completing a series of experiments on different metals previous to the paper being read to the Society, but severe indisposition, and an accident happening subsequently to a part of my apparatus, prevented me completing this at the time I proposed, and I was under the necessity of deferring the prosecution of the inquiry until I should have the leisure of the vacation. This will, I hope, account for my having so long delayed the communication of the results I obtained.

broken, in a manner as invariable as possible. The method by which this might be most readily, and, at the same time, most accurately effected, appeared to me to be, by the falling of the same weight from a constant height, to break the contact; and to renew it by suddenly relieving the cylinder of the tension caused by the same weight. The manner in which this was effected will be best understood by reference to the figure (Plate IV.). M is the marked, and U the unmarked pole of Knight's magnet, belonging to the Royal Society. T L represents the lever, in the cheeks of which the helix A′ B′ containing the iron cylinder is firmly fixed; W is a leaden weight, to which a string $r\,p$ T is attached; this string, after passing through the pulley p, is fixed to the lever at T. The weight W rests upon the door X Y, having hinges at Y, and supported by the pin x, passing through a hook on the door and into the fixed stand. On the observer at the galvanometer withdrawing the pin x, by means of the string $x\,t\,t$, the weight suddenly falls, and at a certain distance stretches the string $r\,p$ T with such force, that acting upon the lever at T, the contact of the iron cylinder with the poles of the magnet is instantly broken. The weight but just touching the ground retains the lever firmly against the frame to which the pulleys $p\,q$ are attached, and keeps the iron cylinder at a certain distance from the poles of the magnet. To the string W $r\,p$ T, another string $r\,q\,s\,s$, passing through the pulley q to the observer at the galvanometer, is attached. On this string being suddenly pulled, the weight is raised, and the lever being relieved of its tension, the contact of the iron cylinder immediately takes place in consequence of the powerful attraction of the magnet. As the magnet has not hitherto been described, I may mention that it consists of four hundred and thirty-seven bar magnets, each fifteen inches long, one inch wide, ·5 inch thick, arranged in two parallel piles, each three bars in length, three in width, and twenty-three in height, connected within an oak box at the further end, not represented in the figure, by a pile of twenty-three bars extending from side to side. The other ends project from the box as represented at M and U, and are partly covered with sheet iron; they are nine inches asunder. The force required to separate the iron cylinder from the poles is equivalent to about sixty pounds, but I do not consider that the magnet has anything near the power which might be given to it by a different arrangement of the magnets.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The galvanometer which I now made use of, although on the same principle, was very different from that already described; in that, the needles are light and rather short; but in this, as it was essential that in no case the needle should be driven past 180°, their weight is considerable; and their length is greater, to ensure greater accuracy of reading. The length of each needle is six inches, its weight 77·5 grains. These needles are connected by their centres, so that the poles of contrary names are vertically over each other, when suspended; the lower needle traverses within the coils of the galvanometer wire, and the upper one above them. The intensity of the upper needle exceeds that of the lower, so as to give a small directive force in the meridian. The compound needle is suspended by a few untwisted fibres of silk. The wire of the galvanometer is copper covered with silk; it is eight hundred and twenty inches in length, and consists of twenty-seven coils on each side of the needle, and one coil which unites them.

If I could possibly avoid doing so, it was not my intention to cut the wires at the several lengths, the intensities of whose action I proposed determining, since if this were once done, I should be precluded from repeating the series of experiments if doubts should arise respecting the accuracy of the results. As a preliminary step, therefore, it became necessary to ascertain how far the intensity of the currents might be influenced by the nature of the contact, when separate pieces were brought together, or by any portion of wire being a part of wires in the magneto-electric circuit, but itself not immediately in that circuit.

Two copper wires, each twelve feet long and ·045 inch in diameter, were each at one of its extremities brought into good contact with a wire one foot long, in contact with the ends of the wires of the compound helix, and at its other extremity, with a wire four feet long, proceeding from the galvanometer. I then made the observations contained in the following Table, where I have not entered the point to which the needle was impelled on making the contact of the iron cylinder with the poles of the magnet, as I found that there was still considerable want of uniformity in this respect, owing to the helix not being at this time firmly secured to the lever in the manner I have described, and in which it was in subsequent experiments.

I.

Circumstances under which the observations were made.	Point to which S. end of needle impelled on breaking contact.
The two wires of twelve feet each, having each one end in good contact with an end of the galvanometer wire, and the other end in contact with the corresponding end of the helix wire.	89° 30′W. 91 15 90 30 90 15
Mean	90 23
Each of the two long wires cut at four feet from the junction with the helix wire; then one inch of the separated parts brightened, brought into contact, and bound round with fine wire ·027 inch in diameter, well brightened.	91 00 91 15 90 30 90 30
Mean	90 49
The ends, instead of being bound as in the last case, were fitted into a brightened sheath of thin copper an inch wide.	91 00 92 00 92 00 90 30
Mean	91 23
The sheath removed, the cut ends just touching, and fine wire bound several times round one and then round the other.	91 00 90 30
Mean	90 45
The fine wire in the last partly unwound, so that the ends of the thick wire are an inch apart, and having an inch of thin wire from the one to the other.	90 00 89 30
Mean	89 45

From these it appears that whether the wire was entire, or the contact made by any of the methods described, there was little difference in the intensity of the current transmitted to the galvanometer; and, consequently, in experiments where different portions of copper wire are successively brought into contact with each other, little error is to be apprehended in the result, from the manner in which the wires are in contact, provided that even moderate care is taken in making the contact.

My next object was to ascertain how far the intensity of the current might be influenced by a portion of wire, not itself in the circuit, but connected with the wire forming the circuit. For this purpose the two wires from the galvanometer were separated from the ends of the connecting wires, and these last wires, at the distance of 4·25 feet from the helix,—that is, just beyond where they had been previously cut and again brought into contact,—were twisted round the galvanometer wires; so that the length of the connecting wire on each side, between the galvanometer and helix, and actually forming part of the magneto-electric circuit, was now 9·25 feet; and 7·75 feet of the wire brought into contact was hanging free at one end, and forming no part of the circuit. The effect of this arrangement on the galvanometer needle having been observed on making and on breaking contact, the free ends of the wires were brought into contact with the connecting wires, very near to the place of first contact, so as to form a large loop on the connecting wire, on each side. The effect of this having been also observed, the wires were separated, where last brought into contact, and the loose part 7·75 feet on each side cut off, so that now the connecting wires were 9·25 feet on each side, and were unconnected with any other wire. The following Table contains the results of these arrangements.

II.

Arrangement of the Wires.	Point to which south end of the needle impelled on contact being	
	Made.	Broken.
Length of connecting wire on each side 9·25 feet, and 7·75 feet of wire connected with each, but free at the other end.	112° E. 115 E.	110° W. 111 W.
Mean	112°	
Length of connecting wire on each side 9·25 feet, and 7·75 feet of wire forming a loop on each connecting wire.	113° 30′ 112 30	112° 15′ 109 00
Mean	111° 49′	
Length of connecting wire on each side 9·25 feet, and the 7·75 feet of wire cut off, so that the connecting wires have no contact with other wires.	112 30 113 30	110 00 109 00
Mean	111° 15′	

The effect of the wire not in the circuit would appear from these to have been but small. If we are not to attribute the differences in the results to errors of observation, arising from slight differences in the manner of making the contact, and also in breaking it*, but to actual differences in the intensity of the current transmitted to the galvanometer, then the part of the wire not actually in the circuit had a tendency to increase the intensity of the current; and this is an effect which might be expected from the mass of metal under induction being somewhat increased without any increase in the length of the circuit. Supposing such to have been the effect of the seven feet of wire, I do not consider that effect to have been proportional to the increase of mass, for had this portion of wire been wound round the connecting wire, so as to form a part of it, I have no doubt the effect would have been much more increased. In some subsequent experiments, made with the view of deciding roughly this point, I found that by winding the middle of each connecting wire once round a copper cylinder sixteen inches long and 0·75 inch thick, so as to be in contact the whole length, the effect at the galvanometer was increased from 135½° to 143°; winding part of one of the wires round a copper cylinder eight inches in diameter and 0·5 inch high, it became 151°; and on placing each of the cylinders on thick coils of copper wire, it was still further increased to 157°. The mean differences in the results in the preceding Table being, however, less than the differences in the results in the same experiment, even in those I am about to describe, and in which the helix was firmly fixed to the lever, so that the contacts were made and broken with great precision, I considered that the effect of the wire not in the circuit might be neglected.

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* The needle traversing at some distance below the graduated circle on which its direction was read, the observation of its direction might be liable to errors arising from parallax, even with the

Having satisfied myself regarding these preliminary points, in order to ascertain the law of intensity as depending on the length of the connecting wire, two soft copper wires, each ·058 inch in diameter and 986·5 inches long, were coiled on a frame, so that the coils of each were an inch apart, and the inner coils of the two wires four inches asunder. There were eleven inches less than twenty coils of each wire, so that each coil was 49·875 inches in length: I had intended that the length of each coil should have been fifty inches, and the whole length of each wire one thousand inches, and I was obliged to make up the twenty coils

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needle remaining stationary, and would be peculiarly so when it was in motion, and the point at which it stopped was to be determined, if means were not adopted to prevent the occurrence of such errors. The upper needle being however always vertically over the other, and the two being exactly of the same form, by reading the direction of the upper, when, one eye being closed, its point appeared precisely over that of the lower, I considered that the observations were as free as possible from errors arising from this source.

by additional pieces, but this was not a matter of any consequence. One coil of each wire was cut off from the outer ends to make the wires of communication from the galvanometer to different parts of the wire on the frame; and half a coil of the inner end of each unwound, to make the connexion with the helix. The ends of the latter were brought into good contact with wires proceeding from the united wires of the helix at the distance 12·25 inches from the ends of these united wires: so that allowing 1·25 inch for contact on each side, when the galvanometer wires were brought into contact with the ends of the outer coils, the length of each wire of communication from the helix to the galvanometer was exactly twenty coils; and by successively bringing them into contact with the first, the third, fifth, seventh, &c. coils, reckoning from the middle of the frame, at points in a line drawn parallel to the sides of the frame and properly determined, the length of each of the wires of communication became successively two, four, six, eight, &c. coils. This will be best understood from the fig. Plate IV. and fig. 4. Plate III., where I G, K H represent the coils of the wire on the frame, which are connected with the helix at A′ B′: A Q, B R the wires from the galvanometer. When the end Q was in contact with the end E, and R with F, then the length of the wire of communication on each side was exactly twenty coils. When the end Q of the wire A Q was in contact with the wire from the helix at C, and likewise the end R with the other wire at D, the length of the wire of communication on each side was two coils. In a similar manner when the contact of the ends Q and R was made with the third coils instead of the first, in the line E F, the wire of communication, on each side, was four coils; and so on.

By this arrangement also two currents of different intensities and in opposite directions might be transmitted, at the same time, to the galvanometer. Thus the wires from the galvanometer A Q, B R being in contact with the wires from the helix at C D, on contact of the helix with the magnet being made or broken, a current in certain directions would pass through a length of two coils of wire on each side: this, without regard to its particular direction on making or breaking contact, I call a direct current, merely to distinguish it from another always in an opposite direction, which I call a reverse current. The wires being united as I have described, and a coil unwound at the part E I, and its end brought in contact with the other wire at D; likewise a coil unwound from KF and brought in contact at C; represented by the dotted lines *e* D, *f* C; then, at the same time that a current passed in one direction through the coils of the galvanometer communicated through two coils of the frame on each side, a current passed also through them in an opposite direction and communicated through twenty coils of the frame on each side: or rather the currents in the wires A′ C, B′ D, each the length of one coil, at the points C and D branched off into two currents; one communicated directly from C to A, D to B through a length of one coil, the other from C to F through eighteen coils to D, and from D to E through eighteen coils to C, and from these points one coil more to the galvanometer at B and A, in an opposite direction to the former current.—The following Table contains the results obtained with different lengths of wire in both cases.

III.

A single current, considered as the Direct Current, transmitted to the Galvanometer.			The difference of two currents transmitted to the Galvanometer, the Reverse current being always through a length of 20 coils of wire on each side.	
Length of the wire forming the connexion between the Helix and Galvanometer, on each side, for the Direct Current.	Deflection of the south end of the Needle on contact being		Deflection of the south end of the Needle on contact being	
	Made.	Broken.	Made.	Broken.
2 Coils	124° 15′ E.	121° 00′ W.	80° 50′ E.	83° 10′ W.
	121 30	121 00	81 30	82 30
	Mean .. 121° 56′.		Mean .. 82° 00′.	
4 Coils	100 30 E.	101 30 W.	51 30 E.	53 45 W.
	98 30	101 45	51 30	53 15
	Mean .. 100° 34′.		Mean .. 52° 30′.	
6 Coils	86 30 E.	87 30 W.	35 30 E.	38 00 W.
	86 00	89 00	36 00	37 30
	Mean .. 87° 15′.		Mean .. 36° 45′.	
8 Coils	78 00 E.	80 00 W.	25 30 E.	27 00 W.
	77 30	78 30	25 30	27 30
	Mean .. 78° 30′.		Mean .. 26° 22′.	
10 Coils	69 15 E.	73 00 W.	17 30 E.	20 15 W.
	70 00	73 45	18 30	20 30
	Mean .. 71° 30′.		Mean .. 19° 11′.	
12 Coils	64 30 E.	67 00 W.	12 45 E.	15 30 W.
	64 15	67 45	12 45	15 15
	Mean .. 65° 52′.		Mean .. 14° 04′.	
14 Coils	59 00 E.	62 45 W.	8 30 E.	11 00 W.
	58 45	62 45	8 30	11 00
	Mean .. 60° 49′		Mean .. 9° 45′.	
16 Coils	55 00 E.	57 15 W.	4 30 E.	7 00 W.
	53 30	58 00	5 00	7 15
	Mean .. 55° 56′.		Mean .. 5° 56′.	
18 Coils	50 30 E.	55 15 W.	1 30 E.	4 00 W.
	51 00	55 00	2 00	4 00
	Mean .. 52° 56′.		Mean .. 2° 52′.	
20 Coils	48 00 E.	50 00 W.		
	47 30	51 00		
	Mean .. 49° 08′.			
0	....		51 00 W.	47 30 E.
			51 00	49 00
			Mean .. 49° 38′.	
Mean of 20 coils Direct, and 20 coils Reverse, 49° 23′.				

In order to deduce from these observations the law according to which the intensity of the current varies, some theoretical investigations are necessary:

but before entering upon these, I must point out some circumstances affecting the observations themselves. In consequence of the great power of the magnet which I employed, it was difficult to place the galvanometer at such a distance, that the needle should be beyond the sphere of its action, or rather that the mere connexion of the poles, or the breaking that connexion, should have no influence upon the needle, independent of that transmitted through the wire. The poles of the magnet being united by the iron cylinder within the helix, the wires of which were not connected with the galvanometer, I carefully adjusted that instrument so that the end of the needle pointed to zero on the graduated ring. On breaking the connexion between the poles, the south end of the needle moved to 2° 15′ E. and settled at 1° 10′ E.; and on again connecting the poles, the needle moved to 1° 10′ W. and settled at 0°. So that merely connecting the poles by the iron cylinder, independent of any effect arising from the current of electricity transmitted through the wire, was equivalent to a force that would drive the needle 1° 10′ W. beyond the zero, 0, of the closed poles; and on the other hand the effect of breaking the connexion was equivalent to a force that would drive the needle 1° 10′ E. beyond the zero (1° 10′ E.) of the open poles. However I might regret the circumstance I could not remedy it, for as the magnet was situated, I could not place the galvanometer at a greater distance than that at which it was, about eight feet; and the magnet itself weighing half a ton might almost be said to be immoveable without taking to pieces and putting up afresh, which, as it consists of nearly five hundred separate bars, would have been a work requiring much more time than I could command. The results to be deduced from the observations will not however be materially affected by this small extraneous force and shifting of the zero; for it will be seen that whether this is taken into the account in the calculation, or not, although the numerical results differ slightly from each other, the conclusions to be drawn from them are the same.

Theoretical Investigation.

From the first discovery of magneto-electricity by Mr Faraday, it appeared that the new force brought into action depended on the motion of the helix, or any conductor, in the vicinity of a magnet, and that its action ceased on the cessation of motion. The current of electricity transmitted from the helix through the galvanometer wire acts upon the needle by impulses during the motion of the helix, and it appears that the force acts at right angles to the wire *. The action upon the needle, to speak accurately, consists in successive impulses continued the whole time during which the helix moves; and in order to estimate the effect upon the needle, it would be necessary to determine the law of the intensity of the current in the helix as depending upon its distance from the magnet and the velocity of its motion. This is an inquiry into which I may enter on some future occasion, as I consider that I see the means by which the object may be accomplished; but it is quite unnecessary for the present purpose. Since in all cases the helix moved through the same space, and came in contact with the magnet with the same velocity, we may refer the action upon the needle, continued during a small but definite portion of time, to a single impulse, which will be the measure of the intensity of the electric current. The intensity of the current will therefore vary as the velocity with which the needle is impelled at the commencement of its motion; and this velocity will be the same as that which it would acquire in descending from its highest to its lowest point, by the force of terrestrial magnetism acting upon it.

Let V be the angular velocity with which the needle begins to move; A the whole arc described by the needle; v the angular velocity corresponding to any arc θ; and m the magnetic intensity acting on either pole in the direction parallel to the wires of the galvanometer, and reduced to the distance 1. Then since the force in the direction of the tangent is $m \sin \theta$, we have

$$v\,dv = -\,m \sin \theta\, d\theta.$$

Integrating from $\theta = 0$ to $\theta = A$

$$V = 2\sqrt{m} \,.\, \sin \tfrac{1}{2} A \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a)$$

Let I′, I″ be the intensities of the currents corresponding to the lengths L′, L″ of the conducting wire on each side, from the helix to the galvanometer, and to the arcs A′, A″; and suppose that the intensity varies inversely as L^n; then

$$\frac{L''^n}{L'^n} = \frac{I'}{I''} = \frac{\sin \frac{1}{2} A'}{\sin \frac{1}{2} A''}$$

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* By placing the needle at right angles to the galvanometer wires, I found that it remained stationary both on making and on breaking contact.

From which

$$n = \frac{\log.\sin\frac{1}{2}A' - \log.\sin\frac{1}{2}A''}{\log.L'' - \log.L'}$$

On deducing the values of *n* from the observed values of A′, A″, &c., I found that they varied from $n = {\cdot}19$, corresponding to $L' = 2$ and $L'' = 4$, to $n = {\cdot}60$ corresponding to $L' = 18$ and $L'' = 20$, increasing pretty regularly, excepting in one instance. These values are far too widely different to be reconcileable with any law of the intensity, depending on a single power of L, even if I could admit that there were great inaccuracies in the observations. Although in drawing conclusions from the experiments, I had wished to abstain as much as possible from theoretical views, yet I had, even in obtaining these values of *n*, adopted, perhaps unconsciously, a particular hypothesis respecting the manner in which the electricity was propagated from one portion of wire to another; and I had, I now consider, very erroneously supposed, that electricity of a certain intensity, depending upon the length of wire through which it had been conducted, was transmitted to the galvanometer wire, which acted upon the needle, and that the length of the galvanometer wire being constant, would have no other influence upon the result than that of a constant coefficient. This would be a necessary consequence of supposing that, whatever might be the length of the wire uniting the two ends of the helix, the intensity of the electricity in the wire at the same distance was always the same; and would follow from the hypothesis, that the electricity is propagated like waves decreasing in magnitude as they increase in distance from the first, which is always of the same magnitude. As, however, I found that such an hypothesis was quite incompatible with the results from observation, I had no hesitation in rejecting it, and was led to consider what circumstance might influence the intensity, independent of the length of wire between the helix and galvanometer. It immediately occurred to me that the length of the galvanometer wire might influence the result in a manner very different from that which I had supposed. If instead of supposing that the intensity in any point in the wire depends alone on its distance from the helix, we suppose that it is influenced by the length of wire beyond it, then the length of the galvanometer wire will affect the results very differently. This supposition is quite compatible with the hypothesis, that the electricity is propagated like waves, decreasing in magnitude as they increase in distance from their first source, the magnitude of the first wave, however, depending on the whole distance to which the waves are propagated; and it would also follow from the hypothesis, that the intensity of the current is the same throughout the whole length of the wire connecting the ends of the helix, and dependent upon that whole length. As it is a matter to be determined by experiment, whether the intensity is the same in every part of the wire, it is not necessary to enter upon the question of which is the more probable hypothesis; but I considered that it was proper to point out the bearing of the experimental results upon different hypotheses that may be adopted[*].

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[*] Since the above was written, in order to determine the effect on the galvanometer needle when placed in different parts of the same circuit, I have made the following experiments. The wires on the frame were first connected with those of the helix and of the galvanometer, so that the length of the conducting wires on each side, from A′ to A and B′ to B, should be twenty coils or very nearly one thousand inches; the connexions were then made so that from A′ to A was two coils, and from B′ to B thirty-eight coils; and likewise so that from A′ to A was thirty-eight coils and from B′ to B two coils: and again similar connexions were made from A′ to B and B′ to A. The results, which are given in the following Table, differ so little from each other, that there can be no doubt of the perfect equality of the action in every part of the wire.

Position of the Galvanometer in the conducting wire		Deflection of the Needle on contact being		Position of the Galvanometer in the conducting wire		Deflection of the Needle on contact being	
A′ to A.	B′ to B.	Made.	Broken.	A′ to B.	B′ to A.	Made.	Broken.
2 Coils	38 Coils	49° 35′ E.	48° 10′ W.	2 Coils	38 Coils	49° 30′ W.	49° 55′ E.
		49 20	48 20			49 10	49 30
		50 00	48 20			49 40	49 50
		Mean . . 48° 57′.				Mean . . 49° 36′.	
20 Coils	20 Coils	49 10 E.	48 30 W.	20 Coils	20 Coils	49 25 W.	49 30 E.
		49 40	48 40			49 20	49 50
		49 20	48 00			49 30	49 40
		Mean . . 48° 53′.				Mean . . 49° 33′.	
38 Coils	2 Coils	49 25 E.	48 15 W.	38 Coils	2 Coils	49 20 W.	50 20 E.
		49 05	48 20			49 25	49 50
		48 50	48 15			49 30	50 05
		Mean . . 48° 42′.				Mean . . 49° 45′.	

This equality of the action on the galvanometer needle, in different parts of the same wire, is compatible with different hypotheses. If opposite currents be supposed to originate in the ends of the wire, from the two poles of the magnet, then the whole of the contrary electricities may be equably distributed throughout the wire; or they may be so distributed that their intensities, proceeding from the ends of the wire connected with the helix, decrease in arithmetical progression, so that, the con-

The galvanometer wire not being of the same diameter as the conducting wire, in order to introduce its length into the expression from which the value of the index n is to be determined, it is necessary that the law of intensity as depending on the diameter of the wire should be known; or this law being unknown, to eliminate n from two equations of the same form as the preceding, and by the observed values of A', A'', &c. and known values of L', L'', to determine, from the resulting equation, the length of wire of the same diameter as the conducting wire, which would be equal in effect to the galvanometer wire. The equation resulting from the elimination of n is of such a form, that to deduce the length in question from it, by means of the observations, would require long and extremely laborious computations. I therefore considered it better to assume a value for n, and, as a test of the correctness of this assumption, to compare the results obtained by means of the observations.

The most simple law according to which the intensity of the electricity can depend upon the length of the wire is, that it varies inversely as that length; and this is also the most probable law; since, whether the electricity is distributed on the surface of the wire or throughout the mass, this would be the law if the same measure of electricity from the same source is given to the wire, whatever may be its length. I therefore assumed this to be the law according to which the intensity varied, and the accordance of the results left no doubt on my mind of the truth of the assumption.

Let us suppose that the wires of the galvanometer are in immediate connexion with those of the helix, so that the intermediate length of wire on each side, $L = 0$; and let 2λ be the length of wire of the same diameter as that coiled round the frame, to which electricity of the same intensity would be communicated, as would, in that case, be communicated to the galvanometer wire. Let ι be the intensity that would be thus communicated to either; and α the arc

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trary actions upon the needle being in opposite directions, their sum would be the same in every part of the wire. This last is an improbable hypothesis, for, independent of other considerations, it would require that the intensity of the electricity, where first communicated to the wire, should vary according to a complicated law, partly directly as the whole length of the wire, and partly inversely as that length. If a single current be supposed to originate in one pole, and to flow through the wire to the other, then the experiments show that the intensity of the current will be the same in every part of its course.

through which the needle would in consequence be impelled; then, the intensity varying inversely as the length of the wire, we shall have,

$$\frac{i}{I} = \frac{L+\lambda}{\lambda} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (b)$$

and consequently

$$\frac{L+\lambda}{\lambda} = \frac{\sin \frac{1}{2} \alpha}{\sin \frac{1}{2} A} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

Eliminating $\sin \frac{1}{2} \alpha$ from two equations of the form (*c*),

$$\frac{L'+\lambda}{\lambda} = \frac{\sin \frac{1}{2} \alpha}{\sin \frac{1}{2} A'} \text{ and } \frac{L''+\lambda}{\lambda} = \frac{\sin \frac{1}{2} \alpha}{\sin \frac{1}{2} A''},$$

we have

$$\lambda = \frac{L'' \sin \frac{1}{2} A'' - L' \sin \frac{1}{2} A'}{\sin \frac{1}{2} A' - \sin \frac{1}{2} A''} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

By combining the observations two and two, different values of λ will be obtained; and if these be substituted in the equation (*c*), the separate observations should give

$$(L + \lambda) \, . \sin \tfrac{1}{2} A = \lambda \sin \tfrac{1}{2} \alpha,$$

a constant quantity. From the mean value of $\lambda \sin \frac{1}{2} \alpha$, thus obtained, the several values of $\frac{1}{2}$ A may be computed and compared with the observations.

I have already stated, that when the poles of the magnet were closed, by the iron cylinder of the helix being applied to them, the zero of the needle was 0° 00′; and that when they were open, it was 1° 10′ E.: it is therefore necessary to inquire how far the results may be affected by this circumstance.

To determine the effect when the needle is impelled by making the contact of the helix with the magnet, let $A_{,}$ be the arc towards east, on the graduated ring, to which the south point of the needle is impelled; $V_{,}$ the initial velocity towards east due to the impulse from the electricity alone; ${}_{,}v$ the velocity towards west due to the magnetic impulse of closing the poles; $\varphi = 1° 10'$: Then

$$V_{,} - {}_{,}v = 2 \sqrt{m} \, . \sin \tfrac{1}{2} A_{,},$$

and $${}_{,}v = 2 \sqrt{m} \, . \sin \tfrac{1}{2} \varphi.$$

Therefore $$V_{,} = 2 \sqrt{m} \, . (\sin \tfrac{1}{2} A_{,} + \sin \tfrac{1}{2} \varphi).$$

When the contact is broken, let ${}_{,}A$ be the arc towards west on the graduated ring; ${}_{,}V$ the initial velocity towards west due to the electricity alone; $v_{,}$ the velocity towards east due to the magnetic impulse of opening the poles: Then the zero being 1° 10′ east,

$${}_{,}V - v_{,} = 2\sqrt{m} \,.\, \sin\tfrac{1}{2}({}_{,}A + \phi),$$

and $$v_{,} = 2\sqrt{m} \,.\, \sin\tfrac{1}{2}\phi.$$

Therefore $${}_{,}V = 2\sqrt{m} \,.\, \{\sin\tfrac{1}{2}({}_{,}A + \phi) + \sin\tfrac{1}{2}\phi\}.$$

Consequently, if ${}_{,}V = V_{,} = V$, and $A_{,} = {}_{,}A + \phi$, taking the mean of the effects due to the making and the breaking of the contact,

$$V = 2\sqrt{m} \,.\, \{\sin\tfrac{1}{4}(A_{,} + {}_{,}A + \phi) + \sin\tfrac{1}{2}\phi\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (a_{,})$$

If $A = \frac{A_{,} + {}_{,}A + \phi}{2}$, then

$$\frac{L+\lambda}{\lambda} = \frac{\sin\frac{1}{2}\alpha}{\sin\frac{1}{2}A + \sin\frac{1}{2}\phi} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c_{,})$$

So that the equation (*d*) will become

$$\lambda = \frac{L''(\sin\frac{1}{2}A'' + \sin\frac{1}{2}\phi) - L'(\sin\frac{1}{2}A' + \sin\frac{1}{2}\phi)}{\sin\frac{1}{2}A' - \sin\frac{1}{2}A''} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d_{,})$$

In the following Table, which contains the results deduced from the observations in Table III., in order to obtain the mean value of λ, I have used only the combinations of the alternate observations, in order to diminish the number of computations, and because the nearer the values of L′ and L″ to each other, the greater would be the effect of any error in the observations.

IV.

	No correction made for the variation of the Zero.						Correction made for the variation of the Zero.				
In coils on the Frame. L	Observed Values. $\frac{1}{2}$ A	Values of L combined in determining λ.	λ	Deduced from the equation (c) when $\lambda = 15{\cdot}10$. $\lambda \sin \frac{1}{2} \alpha$	Computed from (c) when $\lambda = 15{\cdot}10$, and $\lambda \sin \frac{1}{2} \alpha = 14{\cdot}695$. $\frac{1}{2}$ A	Difference between the observed and the computed values of $\frac{1}{2}$ A.	Observed Values. $\frac{1}{2}$ A	λ	Deduced from the equation ($c_{,}$) when $\lambda = 15{\cdot}73$. $\lambda \sin \frac{1}{2} \alpha$	Computed from ($c_{,}$) when $\lambda = 15{\cdot}73$, and $\lambda \sin \frac{1}{2} \alpha = 15.442$. $\frac{1}{2}$ A	Difference between the observed and the computed values of $\frac{1}{2}$ A.
2	60° 58′	L^{i} & L^{iii}	12·97	14·951	59° 15′	−1° 43′	61° 15′·5	13·37	15·726	59° 17′	−1° 58′
4	50 17	L^{i} L^{v}	14·11	14·692	50 18	+ 1	50 34·5	14·60	15·442	50 29	− 5·5
6	43 37·5	L^{i} L^{vii}	14·50	14·558	44 08·5	+ 31	43 55	15·06	15·293	44 23·5	+ 28·5
8	39 15	L^{i} L^{ix}	14·64	14·616	39 30	+ 15	39 32·5	15·27	15·348	39 46	+ 13·5
10	35 45	L^{ii} L^{iv}	14·54	14·665	35 50	− 5	36 02·5	15·05	15·401	36 06	+ 3·5
12	32 56	L^{ii} L^{vi}	15·28	14·733	32 50	− 6	33 13·5	15·88	15·477	33 05·5	− 8
14	30 24·5	L^{ii} L^{viii}	14·74	14·729	30 20	− 5	30 42	15·41	15·480	30 34	− 8
16	27 58	L^{ii} L^{x}	15·03	14·585	28 12	+ 14	28 15·5	15·57	15·346	28 25	− 9·5
18	26 28	L^{iii} L^{v}	16·11	14·752	26 21	− 7	26 45·5	16·75	15·530	26 33	− 12·5
20	24 42	L^{iii} L^{vii}	16·03	14·667	24 45	+ 3	24 51·5	16·76	15·374	24 55·5	+ 4
		L^{iii} L^{ix}	15·96					16·70			
		L^{iv} L^{vi}	16·42					17·16			
		L^{iv} L^{viii}	14·91					15·71			
		L^{iv} L^{x}	15·34					15·89			
		L^{v} L^{vii}	15·90					16·76			
		L^{v} L^{ix}	15·73					16·66			
		L^{vi} L^{viii}	13·11					13·97			
		L^{vi} L^{x}	14·57					15·01			
		L^{vii} L^{ix}	15·48					16·54			
		L^{viii} L^{x}	16·71					16·45			
		Mean..	15·10	14·695			Mean..	15·73	15·442		

Whether we look to the values of the constant $\lambda \sin \frac{1}{2} \alpha$, or to the general agreement between the computed and observed values of $\frac{1}{2}$ A, there can, I think, be no doubt that the intensity of the electricity excited, on making and on breaking contact, varies in the manner I have supposed. There are two exceptions to the very close agreement of the observed and computed values of $\frac{1}{2}$ A, those corresponding to the values of L, 2 and 6. With regard to the second, the difference is perhaps within the limits of the errors to which the observations are liable, without the interference of any particular cause of error; but the same cannot be said of the first, and I certainly consider that some circumstance connected with the manner of making and of breaking contact, of which I was not aware at the time, must have affected the results. In making these observations, the helix was not so securely fixed to the lever carrying it as is represented in the figure (Plate IV.); and, possibly, this being the first observation, the contact may have been made somewhat higher or lower on the face of the magnet than it was after the helix had been drawn into its proper position. This source of error was afterwards obviated by the method of fixing the helix represented in the figure. Had I repeated the whole series of observations, I have little doubt that I should not have found such an anomaly: but previously to being aware of its existence I had no cause for the repetition; and being aware of it, I prefer giving the observations from which I drew the theoretical conclusions, to giving others which might be in closer accordance with theory, but which must have been made after I knew pretty exactly the results required by the theory. Any doubts, however, which might, in consequence, arise respecting the accuracy of the conclusion, that the intensity varies inversely as the length of the wire, were entirely removed by the computation from similar experiments which I made with various lengths of three wires of very different diameters, with a view to determine the law of the intensity as depending on the diameter, and which I shall shortly describe. I should remark, that the results are the same in character whether the correction for the variation of the zero is made or not, there being nearly the same agreement in the several values of the constant $\lambda \sin \frac{1}{2} \alpha$ in both cases, the value however of this constant being increased by the increase in the arc. I may also remark, that the discrepancies in the values of λ, deduced from combining the observations two and two, are not to be considered as arising from the inaccuracy of the formula, but from slight errors to which such observations must be liable, and which become very sensible when they are of opposite kinds in the observations combined: indeed, it is only observations which admit of the utmost accuracy that will bear such a test.

In the following Table, the effects obtained when the difference of two currents is transmitted to the galvanometer are compared with the effects of those currents separately, taking the sine of half the angle of deflection as the measure of the intensity.

V.

The difference of two currents transmitted to the Galvanometer, the reverse current being always through a length of twenty coils of wire on each side.			The direct current alone transmitted to the Galvanometer.		Sin ½ A − sin ½ A″.	Sin ½ A, − (sin ½ A − sin ½ A″)
Length of the wire on each side, forming the direct current.	½ A,	Sin ½ A,	½ A	Sin ½ A		
2 Coils	41° 18′	·66000	61° 15′·5	·87680	·45642	+·20358
4	26 33	·44698	50 34·5	·77246	·35208	+·09490
6	18 40	·32006	43 55	·69361	·27323	+·04683
8	13 29	·23316	39 32·5	·63664	·21626	+·01690
10	9 53	·17164	36 02·5	·58837	·16799	+·00365
12	7 19	·12735	33 13·5	·54793	·12755	—·00020
14	5 10	·09005	30 42	·51054	·09016	—·00011
16	3 16	·05698	28 15·5	·47345	·05307	+·00391
18	1 44	·03025	26 45·5	·45023	·02985	+·00040
20	0 00	·00000	24 51·5	·42038	·00000	

In drawing conclusions from these results, it is to be borne in mind, that the wires carrying the two currents, the difference of whose effects was measured at the galvanometer, did not proceed separately immediately from the wires of the helix to those of the galvanometer, but that the parts A′C, B′D, AQ, BR, from the helix to the contact of the wires from the galvanometer with those on the frame, and also the wires from the frame to the galvanometer, were common to both; and that it was only the parts on the frame, from that contact to the contact with the wire on the other side, which exclusively belonged to the reverse current: for example, if the wires AQ, BR were in contact with the third coils on the frame in the lines CE, DF, and the ends E and F were brought to the same points of contact, but on the opposite sides of the frame, then four coils of wire on each side would be common to the two currents, and sixteen coils, principally on the frame, would exclusively belong to the reverse current. Up to a certain point, namely where the length of wire on each side carrying the direct current is not less than ten coils,—that is, than half the length of the wire carrying the reverse current,—the effect at the galvanometer is very accurately the difference of the effects that would be produced by the currents separately; but beyond this the stronger current has an excess of influence, and this excess increases with the decrease in the length of the wire and consequent increase of the intensity of the current. It may be, that in consequence of the particular arrangement of the contacts of the wires, the full effect of the reverse current was in no case produced; but that the deficiency decreased so rapidly with the increase in the length of the wire carrying the direct current, that when this length became 10 coils, the deficiency was lost in the errors to which the observations are liable. I certainly do not consider that any such deficiency would have been observable had the wires carrying the reverse current proceeded directly from the wires of the helix across to the wires of the galvanometer. As the results from these experiments appeared somewhat anomalous, I have thought it right to give them, although I consider that before a clear explanation can be given of them, it will be necessary to ascertain how far the intensity of the electricity in different parts of the wire may be affected by the peculiar arrangement.

II. *On the Law of Variation of Magneto-electric Intensity in wires of different diameters.*

In order to determine this law, I proposed comparing the effects of three different wires whose diameters should be in the proportion of 4, 2, 1. For this purpose I had a long piece of copper wire divided into two portions, and on a gauge employed by wire-drawers I made choice of three numbers 11, 16, 22, corresponding to holes whose diameters were, as nearly as I could estimate, in the proportion 4, 2, 1. The first portion of wire was passed through the holes Nos. 10, 11; a part of it being cut off, the remainder was annealed and passed successively through the holes to 16; a part of this was also cut off, and the remainder passed through the holes from 16 to 22. The second portion was reserved to be employed as occasion might require; and finding afterwards that I had not sufficient of the wire No. 11, I gave directions that this portion should be reduced to the same diameter. On determining the diameters of all the wires, I, however, found that those of the two pieces of No. 11. were not exactly equal, the diameter of one being ·1247 inch, of the other ·1269 inch: as I employed the two in equal portions, I consider that no sensible error can arise from taking the mean ·1258 inch as the diameter of No. 11. The diameter of No. 16. I found to be ·0633 inch, and the diameter of No. 22, ·0322 inch.

Two lengths of two hundred and fifty inches each, of No. 22, were formed, in separate coils, on the ends of a small frame, and the ends of these coils nearest to the magnet were to be united with the helix wires: two other lengths of one hundred and two inches each, (an inch at each end being allowed for contacts,) were to be united with the wires of the galvanometer, and, at different points, with the wires on the frame, as in the preceding experiments, so that the length of wire conducting on each side from the helix to the galvanometer could be varied from one hundred to three hundred and fifty inches. Precisely the same arrangement was made with the wire No. 16; but in consequence of the original lengths of the two pieces of No. 11, two lengths of three hundred inches each were coiled on a frame, and the contacts between different parts of these and the galvanometer wire were made by two lengths, each of fifty-two inches.

With each of these wires I determined the effects at the galvanometer, at every fifty inches of increase in the length of the conducting wire, from one hundred to three hundred and fifty inches. Previously to commencing the series, I was under the necessity of increasing the directive force of the galvanometer needle, as I found upon the first trial that one hundred inches of No. 11. would cause the needle to pass 180°, in which case the effects could not be compared without taking into account the air's resistance, the necessity of which I considered it better to avoid. The observations which follow cannot therefore be compared with those I have described; and it is for this reason that I mention the circumstance.

The observations are contained in the following Table, where L, as before, is the length of the conducting wire between the helix and galvanometer, its unit of length being fifty inches.

VI.

The unit of length being 50 inches. L	Wire No. 11. Diameter = ·1258 inch. Weight of 1 inch = 27·15 grains. Deflection of the S. end of the Needle on contact being		Wire No. 16. Diameter = ·0633 inch. Weight of 1 inch = 6·80 grains. Deflection of the S. end of the Needle on contact being		Wire No. 22. Diameter = ·0322 inch. Weight of 1 inch = 1·76 grains. Deflection of the S. end of the Needle on contact being	
	Made.	Broken.	Made.	Broken.	Made.	Broken.
2	127° 30′ E.	139° 00′ W.	108° 15′ E.	120° 00′ W.	76° 20′ E.	77° 15′ W.
	127 45	139 15	108 15	118 15	75 40	77 20
	127 00	138 30	108 45	118 00	76 15	77 15
	Mean .. 133° 10′.		Mean .. 113° 35′.		Mean .. 76° 41′.	
3	124 15	135 00	101 00	108 00	63 00	64 00
	124 00	135 30	99 45	108 45	63 15	63 30
	123 00	135 00	99 30	110 15	63 00	63 30
	Mean .. 129° 08′.		Mean .. 104° 33′.		Mean .. 63° 22′.	
4	120 00	128 00	93 45	98 30	54 15	54 20
	120 00	128 15	93 00	98 00	54 30	54 20
	119 30	129 00	93 15	97 30	54 30	53 45
	Mean .. 124° 08′.		Mean .. 95° 40′.		Mean .. 54° 17′.	
5	116 15	124 30	87 15	91 00	48 00	47 30
	117 15	122 30	87 30	91 00	47 45	47 15
	116 30	122 45	87 00	91 15	48 00	48 00
	Mean .. 119° 58′.		Mean .. 89° 10′.		Mean .. 47° 45′.	
6	112 00	120 00	82 00	85 30	42 30	41 30
	111 00	120 00	82 00	85 40	42 30	41 45
	112 30	120 00	82 15	86 00	42 15	42 00
	Mean .. 115° 55′.		Mean .. 83° 54′.		Mean .. 42° 05′.	
7	108 30	116 15	77 00	80 30	38 15	37 30
	110 00	115 30	76 30	80 30	38 15	37 30
	109 00	117 00	76 30	80 30	38 15	37 30
	Mean .. 112° 43′.		Mean .. 78° 35′.		Mean .. 37° 52′.	

These observations could not all be made in the same day, and to guard against any accidental change that might occur in the intensity of the galvanometer needle, I ascertained its time of vibration. I however have not this time for the day on which the observations were made with the wire No. 11, but had previously found the time of vibration to be about twelve seconds, having no record of the exact time; and two days afterwards, when the observations with No. 16. were made, the time of vibration at 10 A.M. was 12·34 seconds, and at 10 P.M. 12·48 seconds, making the mean time 12·41. Seven days afterwards, immediately before making the observations with the wire No. 22, the time of vibration was found to be 12·42 seconds. So that no very sensible change can have taken place during the time of making the observations.

In the following Table, the values of $\frac{1}{2}$A are corrected for the variation of the zero; those of λ are computed from the equation $(d_{\prime})$; those of $\lambda \sin \frac{1}{2}\alpha$ from the equation $(c_{\prime})$; and by means of these, the values of $\frac{1}{2}$A.

VII.

	Wire No. 11.					Wire No. 16.					Wire No. 22.				
L.	$\frac{1}{2}$ A.	λ.	Deduced from Equation $(c_{\prime})$, when $\lambda = 46{\cdot}40$. $\lambda \sin \frac{1}{2}\alpha$.	Computed from Equation $(c_{\prime})$, when $\lambda = 46{\cdot}40$. $\lambda \sin \frac{1}{2}\alpha = 45{\cdot}131$. $\frac{1}{2}$ A.	Difference between the observed and computed values of $\frac{1}{2}$ A.	$\frac{1}{2}$ A.	λ.	Deduced from Equation $(c_{\prime})$, when $\lambda = 14{\cdot}00$. $\lambda \sin \frac{1}{2}\alpha$.	Computed from Equation $(c_{\prime})$, when $\lambda = 14{\cdot}00$. $\lambda \sin \frac{1}{2}\alpha = 13{\cdot}614$. $\frac{1}{2}$ A.	Difference between the observed and computed values of $\frac{1}{2}$ A.	$\frac{1}{2}$ A.	λ.	Deduced from Equation $(c_{\prime})$, when $\lambda = 3{\cdot}76$. $\lambda \sin \frac{1}{2}\alpha$.	Computed from Equation $(c_{\prime})$, when $\lambda = 3{\cdot}76$. $\lambda \sin \frac{1}{2}\alpha = 3{\cdot}6563$. $\frac{1}{2}$ A.	Difference between the observed and computed values of $\frac{1}{2}$ A.
2	66° 53′	61·09*	45·007	67° 16′	+23′	57° 05′	15·71	13·576	57° 13′	+ 8′	38° 38′	3·703	3·6548	38° 39′	+ 1′
3	64 51	50·84	45·220	64 37	−14	52 34	13·94	13·672	52 15	−19	31 59	3·756	3·6494	32 03	+ 4
4	62 21	49·31	45·157	62 17	− 4	48 08	14·05	13·587	48 15	+ 7	27 26	3·856	3·6542	27 27	+ 1
5	60 16	47·63	45·156	60 13	− 3	44 53	14·34	13·601	44 56	+ 3	24 10	3·741	3·6754	24 02	− 8
6	58 15	48·10	45·092	58 20	+ 5	42 15	14·00	13·651	42 06	− 9	21 20	3·757	3·6500	21 22	+ 2
7	56 39	43·18	45·150	56 37	− 2	39 35	12·29	13·595	39 39	+ 4	19 14	3·830	3·6541	19 15	+ 1
		44·67					13·20					3·977			
		44·07					13·83					3·763			
		45·40					13·51					3·784			
		46·32					14·34					4·174			
		44·54					14·87					3·717			
		46·26					14·07					3·759			
		42·83					15·50					3·201			
		46·22					13·91					3·493			
		50·29					12·41					3·881			
Means…		46·40	45·131	$\sin \frac{1}{2}\alpha = {\cdot}97265$			14·00	13·614	$\sin \frac{1}{2}\alpha = {\cdot}97243$			3·760	3·6563	$\sin \frac{1}{2}\alpha = {\cdot}97242$	

If, from the results obtained from the former experiments, by means of the formula

$$\frac{L+\lambda}{\lambda} = \frac{\sin \frac{1}{2}\alpha}{\sin \frac{1}{2}A + \sin \frac{1}{2}\phi},$$

any doubts could be entertained of its accuracy, they must be entirely removed by the results in the foregoing Table: the agreement of the constants among themselves and of the computed with the observed values of $\frac{1}{2}$A are far closer than I had at all anticipated.

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* This result, being so widely different from any of the others, is not included in the mean.

These experiments, however, failed in the principal object I had in view in making them,—the establishing the law according to which the intensity depends on the diameter of the wire; for although they may indicate the law, which I have no doubt, from other experiments, really exists, and satisfactory reasons may be given for the deviations of the results from that law, they certainly do not prove its truth. To apply them to the purpose in view, let $2\lambda_{\prime}$, $2\lambda_{\prime\prime}$, $2\lambda_{\prime\prime\prime}$ be the values of 2λ corresponding to three wires whose diameters are $D_{\prime}$, $D_{\prime\prime}$, $D_{\prime\prime\prime}$, that is, the lengths of those wires which, acting upon the needle like the galvanometer wire, and being in immediate connexion with the helix, shall impel the needle through the same arc α; and suppose that the intensity of the electricity varies directly as the n^{th} power of the diameter of the wire, and inversely as its length: then μ being a constant,

$$\frac{D_{\prime}^{\,n}}{\lambda_{\prime}} = \mu \sin \tfrac{1}{2}\alpha, \quad \frac{D_{\prime\prime}^{\,n}}{\lambda_{\prime\prime}} = \mu \sin \tfrac{1}{2}\alpha, \quad \frac{D_{\prime\prime\prime}^{\,n}}{\lambda_{\prime\prime\prime}} = \mu \sin \tfrac{1}{2}\alpha \quad . \quad . \quad . \quad . \quad . \quad (e)$$

We have therefore,

$$\frac{\lambda_{\prime}}{\lambda_{\prime\prime}} = \frac{D_{\prime}^{\,n}}{D_{\prime\prime}^{\,n}}, \quad \frac{\lambda_{\prime}}{\lambda_{\prime\prime\prime}} = \frac{D_{\prime}^{\,n}}{D_{\prime\prime\prime}^{\,n}}, \quad \frac{\lambda_{\prime\prime}}{\lambda_{\prime\prime\prime}} = \frac{D_{\prime\prime}^{\,n}}{D_{\prime\prime\prime}^{\,n}} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (f)$$

and consequently

$$n = \frac{\log.\lambda_{\prime} - \log.\lambda_{\prime\prime}}{\log.D_{\prime} - \log.D_{\prime\prime}}, \quad n = \frac{\log.\lambda_{\prime} - \log.\lambda_{\prime\prime\prime}}{\log.D_{\prime} - \log.D_{\prime\prime\prime}}, \quad n = \frac{\log.\lambda_{\prime\prime} - \log.\lambda_{\prime\prime\prime}}{\log.D_{\prime\prime} - \log.D_{\prime\prime\prime}}$$

Taking $D_{\prime} = \cdot 1258$, $D_{\prime\prime} = \cdot 0633$, $D_{\prime\prime\prime} = \cdot 0322$; $\lambda_{\prime} = 46\cdot 40$, $\lambda_{\prime\prime} = 14\cdot 00$, $\lambda_{\prime\prime\prime} = 3\cdot 76$; the values of n are 1·744, 1·844, 1·945; the mean, 1·844.

From these it would be inferred, either that the intensity did not vary as any single power of the diameter, or that, varying as the square of the diameter, there were circumstances connected with the experiments themselves which caused the aberrations from the law, and that their influence increased with the diameter of the wire. Having ascertained from experiments made immediately following these, that the intensity varies as the square of the diameter of the wire, I was led to consider what could be the cause of this discrepancy in the results. It is clear that, according to this law, the values of $\lambda_{\prime}$ and $\lambda_{\prime\prime}$ are too small as compared with that of $\lambda_{\prime\prime\prime}$, and that the deficiency of $\lambda_{\prime}$ is by much the greatest. If the arcs of deflection were increased, the values of λ would be so likewise; and the greater the increase in the one case, the greater would it be in the other. It therefore appeared not improbable, as the resistance of the air on the needle would produce a greater diminution in the arcs the larger they were, that this was the cause.

Assuming then $\lambda_{,}$, $\lambda_{,,}$, $\lambda_{,,,}$ to be as $D_{,}^2$, $D_{,,}^2$, $D_{,,,}^2$; that is, making no change in $\lambda_{,,,}$, and assuming $\lambda_{,} = 59·608$, $\lambda_{,,} = 15·092$; taking $\sin \frac{1}{2} \alpha = ·97250$, the mean of the values obtained, I computed the values of $\frac{1}{2}$ A corresponding to the different values of L. Comparing these with the observed values, I found, in both cases, that as the arcs decreased, the differences increased. If instead of assuming $\lambda_{,} \sin \frac{1}{2} \alpha = 59·608 \times ·97250$ and $\lambda_{,,} \sin \frac{1}{2} \alpha = 15·092 \times ·97250$, I determined their values from the observations; and then computed the values of $\frac{1}{2}$ A from the mean values so determined, the larger arcs were all in defect, and the smaller in excess. So that in either case the result was the reverse of what would arise from diminution of the arc by the air's resistance, which therefore clearly could not be the cause of the discrepancy in question. The results of these assumptions and computations are contained in the following Table.

L.	No. 11. $\begin{cases}\lambda = 59·608. \\ \lambda \sin \frac{1}{2} \alpha = 57·969.\end{cases}$			No. 11. $\begin{cases}\lambda = 59·608. \\ \lambda \sin \frac{1}{2} \alpha = 56·854.\end{cases}$			No. 16. $\begin{cases}\lambda = 15·092. \\ \lambda \sin \frac{1}{2} \alpha = 14·677.\end{cases}$			No. 16. $\begin{cases}\lambda = 15·092. \\ \lambda \sin \frac{1}{2} \alpha = 14·427.\end{cases}$		
	$\frac{1}{2}$ A.		Difference.	$\lambda \sin \frac{1}{2} \alpha$.	$\frac{1}{2}$ A.	Difference.	$\frac{1}{2}$ A.		Difference.	$\lambda \sin \frac{1}{2} \alpha$.	$\frac{1}{2}$ A.	Difference.
	Computed.	Observed.					Computed.	Observed.				
	° ′	° ′	° ′		° ′	° ′	° ′	° ′	° ′		° ′	° ′
2	68 33	66 53	+1 40	57·289	65 53	−1 00	58 03	57 05	+0 58	14·522	56 30	−0 35
3	66 19	64 51	+1 28	57·310	63 53	−0 58	53 14	52 34	+0 40	14·550	51 56	−0 38
4	64 19	62 21	+1 58	56·992	62 05	−0 16	49 20	48 08	+1 12	14·412	48 12	+0 04
5	62 30	60 16	+2 14	56·759	60 26	+0 10	46 05	44 53	+1 12	14·383	45 09	+0 16
6	60 51	58 15	+2 36	56·458	58 55	+0 40	43 17	42 15	+1 02	14·396	42 22	+0 07
7	59 20	56 39	+2 41	56·318	57 30	+0 51	40 51	39 35	+1 16	14·302	40 00	+0 25
			Mean	56·854					Mean	14·427		

Whatever may be the law according to which the intensity depends upon the diameter of the wire, if the wire in the coils of the galvanometer wire act merely as a multiplier, without any effect of accumulation or diminution arising from the action of the contiguous wires upon each other, and the degree of which action may depend on the intensity itself, then the values of $\lambda_{,}$, $\lambda_{,,}$, $\lambda_{,,,}$ should be half the length of the galvanometer wire, reduced, according to that law, to wires whose diameters are $D_{\prime}$, $D_{\prime\prime}$, $D_{\prime\prime\prime}$. The diameter of the galvanometer wire is ·0380 inch, and the whole length of it in the circuit 819 inches; so that, if λ_0 is half its length in the unit of length, 50 inches, $\lambda_0 = 8{\cdot}19$. If its diameter is D_0, then

$$\lambda_{\prime} = \lambda_0 \cdot \frac{D_{\prime}^{n}}{D_0^{n}}, \quad \lambda_{\prime\prime} = \lambda_0 \frac{D_{\prime\prime}^{n}}{D_0^{n}}, \quad \lambda_{\prime\prime\prime} = \lambda_0 \cdot \frac{D_{\prime\prime\prime}^{n}}{D_0^{n}}.$$

Taking the value of n in the first equation to be 1·844, as that deduced from wires whose diameters are most nearly $D_{\prime}$ and D_0; and in the second and third, for the same reason, to be 1·945; we shall have $\lambda_{\prime} = 74{\cdot}5$, instead of 46·4, deduced from the observations; $\lambda_{\prime\prime} = 22{\cdot}1$, instead of 14; and $\lambda_{\prime\prime\prime} = 5{\cdot}93$ instead of 3·76. Also if the values of λ_0 are deduced from those of $\lambda_{\prime}$, $\lambda_{\prime\prime}$, $\lambda_{\prime\prime\prime}$ determined from the observations, these values will be 5·222, 5·189, 5·189, instead of 8·19, the true value. It is therefore clear from all these results, that the galvanometer wire acts as a wire of less than its real length, that is, that there is an increase of intensity in it, in consequence of the action of the coils upon each other. Had the value of λ_0 been deduced from the values of $\lambda_{\prime}$, $\lambda_{\prime\prime}$, $\lambda_{\prime\prime\prime}$, on the supposition that $n = 2$ in the above equations, those values would have been 4·234 from the first equation, 5·045 from the second, 5·237 from the third, instead of 8·19: showing not only that an accumulation of electricity arising from the action of the coils of the galvanometer on each other takes place, but that the accumulation becomes greater as the intensity increases. To this accumulation I consider that the discordances in the values of n are to be attributed, and that these values do not give the number 2, nearly as their mean.

As soon as I was aware that the length of the galvanometer wire must enter into the expression for the intensity, I foresaw the difficulties that would arise in drawing theoretical conclusions from the observations with different wires; and I had proposed making use of a galvanometer having only one turn of the wire, which should be the same as the other part of the conducting wire, and which must therefore be removable in order to determine the effects of different wires: but I felt that so much must depend upon the different wires occupying precisely the same position with respect to the needle, that I feared the changing the wire of the galvanometer would be a great source of error in the observations, and therefore gave up the idea.

I have already stated that I ascertained, by other experiments, that the intensity varies as the square of the diameter of the wire. The method which I adopted in these, was that described in the beginning of this paper, and consisted in determining what length of wire of one diameter would exactly destroy the effect of a given length of another wire of a different diameter. In this manner I had proposed to compare the wire No. 11. with each of the others, and also these with each other; but the experiments which I have described showed that three hundred and fifty inches, the whole length of No 11, was not sufficient to reduce its action to that of eighty-five or eighty-six inches of No. 22, the shortest length that would reach from the helix to the galvanometer. I could therefore only compare No. 11. with No. 16, and again this with No. 22. For this purpose, both the frames on which the wires No. 11. and No. 16. were coiled, were placed between the magnet and galvanometer, as in fig. 5, care being taken that the wires should be kept well apart from each other. Two corresponding ends of the wires of each frame were united with the helix at A′, and the other two corresponding ends, in like manner, with the helix at B′. Fifty-two inches of No. 11. (an inch at each end being allowed for contact,) and likewise fifty-two inches of No. 16. were each at one end in contact with the galvanometer wire at A, and similarly fifty-two inches of each were in contact with the other galvanometer wire at B. The ends of No. 11. not in contact with the galvanometer wire, were brought into good contact with the corresponding ends of No. 11. on the frame, so that the helix and galvanometer were connected by three hundred and fifty inches of No. 11. from A′ to A, and three hundred and fifty inches from B′ to B. The ends of No. 16. proceeding from A and B, were brought into good contact with No. 16. on the frame, but on contrary ends of it; so that the helix at A′ was connected with the galvanometer at B, and likewise the helix at B′ with the galvanometer at A, by No. 16. In other words, what I have, for the sake of distinction, termed the direct current, was transmitted to the galvanometer by three hundred and fifty inches of the wire No. 11. on each side, and what I have termed the reverse current was transmitted by No. 16.

I first reduced the length of No. 16. on each side to one hundred inches, and as there was a motion of the needle of about 5°E. on making contact, and of about 4°W. on breaking it, I further reduced the length to ninety inches. There still appearing a small motion easterly on making contact, and a slight westerly motion on breaking it, I reduced the length to eighty-nine inches, when the wires appeared exactly to counteract each other's effects. I therefore considered that by this method it appeared that three hundred and fifty inches of No. 11. was exactly equivalent in effect to eighty-nine inches of No. 16. I then determined separately the effects at the galvanometer, when A′ and A, B′ and B were connected by three hundred and fifty inches of No. 11; and when A′ and B, B′ and A were so connected; and in like manner when similar connexions were made with eighty-nine inches of No. 16. Observations, precisely similar to these, were made with the wires No. 16. and No. 22, excepting that as ninety inches of No. 22. was found to be equivalent to three hundred and fifty inches of No. 16, the effects of these lengths of those wires were determined. These observations are contained in the following Table.

VIII.

The Wires No. 11. and No. 16. compared.				The Wires No. 16. and No. 22. compared.			
350 inches of No. 11. from U to A, and 350 inches from M to B. 89 inches of No. 16. from U to B, and 89 inches from M to A.				350 inches of No. 16. from U to A, and 350 inches from M to B. 90 inches of No. 22. from U to B, and 90 inches from M to A.			
Motion of the S. end of the Needle on contact being				Motion of the S. end of the Needle on contact being			
Made.		Broken.		Made.		Broken.	
From 1° 10′E. no motion E. then slowly to 1° 00′W. The same, 1° 10′E. to 1° 00′W. The same, 1° 10′E. to 1° 00′W.		From 0° 00′ no motion W. then slowly to 2° 00′ E. The same, 0° 00′ to 2° 10′ E. The same, 0° 00′ to 2° 10′ E.		From 1° 00′E. no motion E. then slowly to 0° 50′W. The same, 1° 10′E. to 1° 00′W. The same, 1° 10′E. to 1° 00′W.		From 0° 00′ no motion W. then slowly to 2° 00′E. The same, 0° 00′ to 2° 15′E. The same, 0° 00′ to 2° 00′E.	
No. 11. alone.		No. 16. alone.		No. 16. alone.		No. 22. alone.	
350 inches from U to A, and 350 inches from M to B.		89 inches from U to A, and 89 inches from M to B.		350 inches from U to A, and 350 inches from M to B.		90 inches from U to A, and 90 inches from M to B.	
Contact made.	Contact broken.	Contact made.	Contact broken.	Contact made.	Contact broken.	Contact made.	Contact broken.
118° 00′E.	123° 15′W.	117° 30′E.	123° 20′W.	79° 10′E.	81° 00′W.	78° 45′E.	80° 30′W.
118 40	123 00	117 30	123 50	79 20	80 30	78 40	80 45
118 00	122 30	117 40	123 45	79 35	80 00	78 45	80 25
Mean 120° 34′		Mean 120° 36′		Mean 79° 56′		Mean 79° 38′	
350 inches from U to B, and 350 inches from M to A.		89 inches from U to B, and 89 inches from M to A.		350 inches from U to B, and 350 inches from M to A.		90 inches from U to B, and 90 inches from M to A.	
Contact made.	Contact broken.	Contact made.	Contact broken.	Contact made.	Contact broken.	Contact made.	Contact broken.
123° 00′W.	116° 20′E.	122° 00′W.	115° 00′E.	81° 15′W.	78° 30′E.	81° 15′W.	78° 30′E.
123 00	116 20	122 15	115 00	81 20	78 30	81 30	78 30
123 15	116 20	122 30	115 30	81 20	79 00	81 30	78 45
Mean 119° 43′		Mean 118° 43′		Mean 79° 59′		Mean 80° 00′	
Means 120° 08′		119° 39′		Means 79° 57′		79° 49′	

The motion of the needle, when three hundred and fifty inches of No. 11. acted, according to the differential arrangement, against eighty-nine inches of No. 16, and also when three hundred and fifty inches of No. 16. acted against ninety inches of No. 22, being precisely that which, as I have before described, was due to the closing and opening of the poles of the magnet, shows that the opposite currents of electricity were precisely equal in both cases; that is, that the intensity in three hundred and fifty inches of No. 11. was precisely equal to that in eighty-nine inches of No. 16; and the intensity in three hundred and fifty inches of No. 16. equal to that in ninety inches of No. 22. And if the results obtained by this mode of experimenting admit of confirmation, they are fully confirmed by a comparison of the results obtained with these lengths of the wires arranged singly. With regard to the latter results, I may notice that they all exceed those which I had before obtained with the same lengths of the wires, and which are given in Table VI. As my object in making these experiments was to obtain the relative results with the two wires, my attention was not at the time particularly drawn to this circumstance, and I can now assign no particular cause for this want of agreement, but find, on referring to the experiments, that the results which I obtained at this time with other lengths of the wires possess the same character, although an interval of only a few days had elapsed since the former observations had been made.

If the intensity vary directly as the n^{th} power of the diameter of the wire, and inversely as its length, then

$$\frac{I_{,}}{I_{,,}} = \frac{D_{,}^{n}}{L_{,}+\lambda_{,}} \times \frac{L_{,,}+\lambda_{,,}}{D_{,,}}$$

and if $I_{,} = I_{,,}$

$$D_{,}^{n}(L_{,,}+\lambda_{,,}) = D_{,,}^{n}(L_{,}+\lambda_{,})$$

also $$D_{,}^{n}\lambda_{,,} = D_{,,}^{n}\lambda_{,}$$

therefore $$D_{,}^{n}L_{,,} = D_{,,}^{n}L_{,}$$

whence $$n = \frac{\log. L_{,} - \log. L_{,,}}{\log. D_{,} - \log. D_{,,}}$$

Substituting 350 for $L_{,}$, 89 for $L_{,,}$, ·1258 for $D_{,}$ and ·0633 for $D_{,,}$, we have $n = 1·9937$;
and again, 350 for $L_{,}$, 90 for $L_{,,}$, ·0633 for $D_{,}$ and ·0322 for $D_{,,}$, we have $n = 2·0093$

Mean .... $n = 2·0015$

There can be no doubt from these results that, in wires of a given length, the intensity of magneto-electricity varies as the squares of the diameters. The law therefore which has been established, is, that in wires of different lengths and different diameters, the intensity of magneto-electricity, or, in other words, the conducting power, varies as the squares of their diameters directly, and as their lengths inversely. Or we may say, that the intensity or conducting power varies as the mass or weight directly, and the square of the length inversely.

Different laws according to which volta-electricity and thermo-electricity are conducted by wires, have resulted from the investigations of different philosophers. Davy found that[*], "when different portions of the same wire, plunged in a non-conducting fluid, were connected with different parts of the same battery equally charged, their conducting powers appeared in the inverse ratio of their lengths;" and that the conducting power of a wire for electricity was nearly as the mass,—that is, when the wire was of a given length; and also, that it was independent of surface. The same law was subsequently shown by M. Becquerel to be true, by a totally different method[†]. M. Becquerel thus states the law which he found to hold: "Ainsi pour obtenir la même conductibilité dans deux fils de même métal, il faut que leurs poids soient proportionnels aux carrés de leurs longueurs, au bien que leurs longueurs soient dans le rapport des sections des fils." The law as thus stated would hold whether the conducting power be, as the diameter directly and the square root of the length inversely, or as the square of the diameter directly and the length inversely. M. Becquerel has, however, previously shown that when the diameter is the same, the conducting power is inversely as the length; and it therefore follows that in general it is as the square of the diameter directly and length inversely.

From experiments described in a very interesting paper, "On the Development of Electro-Magnetism by Heat[‡]," Professor Cumming states that "so far as these experiments (given as mere approximations,) may be depended on, the conducting power of copper wires of considerable length varies as $\frac{D}{\sqrt{L}}$[§]."

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[*] Philosophical Transactions, 1821, p. 435.

[†] Annales de Chimie, 1826, p. 425.

[‡] Cambridge Philosophical Transactions, 1823.

[§] Manual of Electro-dynamics.

Although the deviations of the magnetic needle, obtained with wires 32, 16, 8, 4 feet in length*, give values of the index of L, varying from $-\cdot 394$ to $-\cdot 655$, yet they give a mean $-\cdot 536$, which may be considered as an approximation to $-\frac{1}{2}$. Some of the experiments however with wires of different diameters, give results approximating to 2 rather than to 1 as the index of D; thus with eight feet of wire $\frac{1}{20}$th of an inch in diameter, the deviation was 15½°; and with eight feet of wire $\frac{1}{37}$th of an inch, the deviation was 6½°. These would give 1·68 as the index of D, which might be considered as an approximation to $\frac{5}{3}$. And again,

$$\left.\begin{array}{l}\text{† “Six wires of } \frac{1}{30}\\ \text{Four wires of } \frac{1}{130}\\ \text{Three wires of } \frac{1}{30}\end{array}\right\}\text{ gave the same deviations as one of }\left\{\begin{array}{l}\frac{1}{15}\\ \frac{1}{60}\\ \frac{1}{15}\end{array}\right.\begin{array}{l}\text{each } 16\frac{1}{2}\text{ inches long}\\ \\ \text{each } 1\frac{1}{2}\text{ inch long.”}\end{array}$$

These give respectively 2·58, 1·79, 1·58 as the index of D, or 1·98 as the mean.

In a preceding paper‡, Professor CUMMING states, that "in using large wires, the surface alone does not transmit this magnetic influence." Had this remark been followed out in its consequences, it must have led to the law of the square of the diameter. The law, however, deduced by Professor CUMMING, is in accordance with the enunciation of M. BECQUEREL's which I have quoted.

From experiments with conducting wires varying in length from eight hundred and thirty-eight feet to ninety-eight feet, Mr. BARLOW infers, that the electro-magnetic effect from a battery varies very nearly inversely as the square root of the length of the conducting wire §. But the results computed on this hypothesis differ so widely from the observations, (as much in one instance as 1° 59′ in 4° 30′, in another 2° 16′ in 8° 31′, and very commonly more than 1° 30′ in 8° or 9°,) that it appears to me these observations can scarcely be adduced in support of the probability of the existence of such a law. Experiments described in the same paper, with wires of the same length, but of different weights, would lead to the conclusion, that, although the effect increases with the diameter of the wire, yet the increase is extremely small; since in copper wires of the same length, and varying in weight from seventeen grains to one thousand five hundred and ninety grains, the deflection of the needle, measuring the effect, only increased from 25° to 38°; and with brass wires varying in weight from thirty-eight grains to three thousand seven hundred and seventy grains the deflection only increased from 26° to about 32°.

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* Cambridge Philosophical Transactions, 1823, p. 63.

† Ibid. p. 72.

‡ Camb. Phil. Trans. 1821, p. 277.

§ Edinburgh Philosophical Journal, 1825.

Considering, then, that the law according to which volta-electricity is conducted is that laid down by Sir HUMPHRY DAVY and M. BECQUEREL, it appears that magneto-electricity is conducted according to the same law; or rather, I would say, that the intensity of the electricity excited in the conducting wires varies, in the two cases, according to the same law. I am disposed to think that the intensity of thermo-electricity varies likewise according to the same law, and that the reason that the experiments hitherto made have not led to the establishment of this law has been, the difficulty of separating the effects produced on the wire by the conduction of the agent from the power primarily excited in it. Without having made the trial, it is difficult to say how far the arrangement which I have made use of may be applied with success to the determination of the law according to which thermo-electricity is excited in wires of different lengths and different diameters. I foresee difficulties, arising from the circumstance that heat being applied at the junction of the crossed wires of different diameters, with the two different metals, a secondary action would ensue. These difficulties may not however be insurmountable, and would perhaps be overcome by dispensing altogether with the thermo-electric battery of two metals. Should I have leisure, I may make the attempt, but at all events it is my intention to apply the principle of the differential arrangement to volta-electricity, as I consider that, where applicable, it is capable of giving results to a degree of accuracy hitherto unattained.

From the law which has been established we may conclude, that whatever may be the length of a wire of an invariable diameter, it will in all cases receive the same measure of electricity from the first source, or that the whole amount of electricity excited in it will be the same: that where the section perpendicular to the direction in which the electricity may be said to be propagated is variable, the amount is proportional to the section, so that every portion of this section receives the same measure of electricity. Whether magneto-electricity consists in a current of imponderable matter, or is a latent power brought into action during the motion of a body in the neighbourhood of a magnet, it would appear that it is developed with the same intensity in every part; but this it cannot be at the same instant, since in one case, time must be required for its progress from one point to another, and in the other, for its development. If the power consist in any peculiar arrangement of particles, still time must be required to produce that arrangement, which must proceed from particle to particle, the action on the more remote particles only taking place through the intervention of the nearer. The velocity of the current, the rapidity with which the power is brought into action, or with which the arrangement takes place, whichever it be, may be quite beyond any means which can be devised for their determination; and this they may be, not only without surpassing velocities of which astronomical phenomena give us a conception, but even falling very short of them; but although they may surpass them as much as these do any which are obvious to our senses, still the action on the remote parts of the wire cannot be absolutely simultaneous with that on the parts in the immediate neighbourhood of the magnet. The electricity in the wire must consequently be considered as in motion, however short may be the time of its passage from the nearest to the most remote point.

III. *On the Intensity of the Electricity excited in different metals.*

I have already referred to the manner in which I proposed pursuing this inquiry. For the comparison of the intensities in copper, zinc, tin, iron and lead, I made use of wires about one twentieth of an inch in diameter, and for that of the finer metals, gold, silver, platinum, with copper, of wires of about three hundredths of an inch. The former were all drawn through the same hole, as were also the latter. The copper, zinc, tin, iron and lead, are the ordinary metals of commerce; the gold is unalloyed, and the silver is standard.

Two lengths, of fifty inches each, were accurately measured of each of the larger wires, exclusive of an inch and a half marked off at each end for contact. The wires of the two metals under comparison were arranged as in fig. 3; the wire of the metal which had the greatest intensity always connecting A′ with A, and B′ with B, or, as I have before designated it, carrying the direct current, and the wire of the metal of weaker intensity connecting A′ with B, and B′ with A, or carrying the reverse current. The length of wire from D′ to D and from C′ to C was in all cases fifty inches. The length of the wire from D′ to C and from C′ to D was also in the first instance fifty inches. The effect at the galvanometer having been observed, the distance between the contacts on these last wires was reduced five inches, and the effect again observed: and I continued reducing the lengths of these wires by five inches and observing the effects, until the deflection of the galvanometer needle was reduced to 4° or 5°. I then observed at smaller intervals, and determined the length of the wires of the metal having the weaker intensity, which was equivalent in effect to fifty inches of the other, on each side, either by no motion taking place in the galvanometer needle on the contact of the iron cylinder with the magnet taking place, or on that contact being broken; or otherwise, by finding two lengths of the shorter wire which gave opposite deflections of the needle, and dividing the interval between them in the proportion of these deflections The galvanometer made use of in this case was the extremely sensible one with light needles, already described. The effect on this, of closing the poles of the magnet with the iron cylinder, was to make the needle vibrate from $1\frac{1}{2}$ E. to $1\frac{1}{2}$ W., and the effect of opening them, to vibrate from 0° to 3° E.

In this manner each of the metals, zinc, tin, iron, lead, was compared with copper; tin, iron, lead, with zinc; iron, and lead with tin; and lead with iron. With all the metals except lead these deflections of the needle decreased pretty regularly as the wire of the weaker was shortened; but with the lead, on some occasions, I found that when I had decreased the length of its wires, an increased effect of the same kind as before, took place; and on others, when on a first trial of making contact, and also on breaking it, the needle remained immoveable; on a second it was deflected 5° or 6° in a direction indicating that the lead wire was still too long. When this wire had been slightly diminished in length, the needle was deflected decidedly in the opposite direction; so that it was hardly possible to say what was the correct length required of the lead wire. At first I attributed this anomalous effect to some of the wires approaching each other too nearly, but at length I was satisfied that it proceeded from the metallic contact between the lead and the other metals gradually becoming imperfect by the oxidation of the surface of the lead. The effect is so singular, that I give the observations which pointed out to me the true cause. From previous observations I considered that about eleven inches of lead wire would be equivalent to fifty inches of zinc wire, but had found this anomalous effect to take place with these lengths. I therefore brightened the ends of the wires on each side at these distances, and again bound them together with bright copper wire, the lengths of the zinc wires carrying the direct current being each fifty inches, and of the lead wires carrying the reverse current each 11·2 inches.

Time.	Motion of the Needle on the contact of the Helix with the Magnet being		Time.
	Made.	Broken.	
h m			h m
	Steady, then 2° W.	2° W. then 6° E.	
	2° E.	Steady, then 3 E.	
	3 E.	3 W.	
	7 E.	4 W.	4 12
4 20	14 E.	10 W.	
	16 E.	14 W.	
	25 E.	14 W.	
	15 E.	7 W.	
	17 E.	12 W.	4 36
	28 E.	14 W.	5 54
	30 E.	28 W.	
	35 E.	50 W.	7 38
	Changed the Positions of the wires, but their contacts untouched.		
	45° E.	38° W.	
	The wires nearest to the Galvanometer unbound, brightened, and again bound.		
	30° E.	30° W.	

Observations continued.		Time.
The wires nearest to the Helix unbound, brightened, and rebound.		
Contact Made.	Contact Broken.	
		h m
Steady, then 1° W.	1° W. then E.	
1½° E. to 2° E. then W.	3 W. then E.	8 21
4 E.	1 W. then E.	-
2 E.	2 W. then E.	9 22
28 E.	29 W.	18 40
The wires were accidentally shaken.		
10° E.	10° W.	
5 E.	5 W.	
One set of wires raised six inches above the other.		
Steady, then 3° W.	3° W.	19 20
5° E.	3 W.	19 27
10 E.	11 W.	20 05
22 E.	21 W.	21 00
At the places of contact, the wires were pressed so that the binding wire entered the lead, but the wires were not moved.		
3° E.	2° W.	21 5
3 E.	2 W.	21 9
4 E.	2 W.	21 11
6 E.	4 W.	21 17

The following are similar observations with copper and lead wires; the length of the lead wires on each side being 5·9 inches, and of the copper wires, as usual, fifty inches.

Time.	Motion of the Needle on the contact of the Cylinder with the Magnet being		Time.
	Made.	Broken.	
h m			h m
22 10	Steady, then 1° W.	Steady, then 2° E.	
	Steady, then 1½ W.	1° W. then 3 E.	
	½° E. then 2 W.	2 W.	22 17
	Steady, then 2 W.	5 W.	22 20
	8 E.	12 W.	24 25
	12 E.	13 W.	24 29
	Pressed all the joints of the wires.		
	Steady, then ½° W.	Steady, then 2° E.	24 31
	Steady,	Steady, then 2 E.	24 32

From the former set I inferred that, when all the contacts were good, 22·4 inches of lead wire in the arrangement was equivalent to one hundred inches of zinc wire; and from the latter that 11·8 inches of lead wire was equivalent to one hundred inches of copper wire.

I had no doubt, after making these observations, that the anomalies I had previously observed, arose from the contact between the lead and the other metals becoming imperfect after a very short time; and I therefore determined the length of the lead wires to be that which, on the observation being made immediately after the wires had been bound together, produced no motion in the galvanometer needle. In consequence however of this, I do not place the same confidence in the results obtained with the lead wire which I do in the others*. Nothing of this kind was observed with any of the other metals, though, probably, it might take place if the wires were left for a considerable time in contact. With the lead I noticed the effect principally when it was compared with iron, with zinc, and with copper.

The relative lengths of the wires of different metals, as determined by the observations, are arranged in the following Table.

IX.

Metals taken as Standards, the lengths of their wires being 100 inches.	Metals compared, and the lengths of their wires corresponding to 100 inches of the wire taken as the standard.				
	Copper.	Zinc.	Tin.	Iron.	Lead.
	inches.	inches.	inches.	inches.	inches.
Copper	100	52·0	25·5	22·8	11·8
Zinc	......	100·0	49·08	42·0	22·4
Tin	......	......	100·00	89·4	52·0
Iron	......	......	......	100·0	62·0

In order to reduce the relative lengths of two wires determined by experiment, to the standard of one hundred inches of copper wire, it is necessary either to assume one of them of the length determined by comparison with the copper, or to assume their sum as so determined, and divide it in the ratio of the observed lengths. As the second method tends to distribute the errors of the first determination equally between the two reduced lengths, I adopt it in the reductions, made in the following Table, of the above observations.

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* I might have avoided this uncertainty, by making the ends of the wires just touch the surface of mercury contained in four cups, properly placed for the purpose; and this in many cases may be the most convenient way of making the differential arrangement of the wires: but in making these experiments it was an object to avoid cutting the wires, which must have been done had mercury been used.

X.

Metals with which others are compared.	Metals compared, and the lengths of their wires reduced to the Standard of 100 inches of copper wire.				
	Copper.	Zinc.	Tin.	Iron.	Lead.
	inches.	inches.	inches.	inches.	inches.
Copper	100	52·0	25·5	22·8	11·8
Zinc	100	....	25·52	22·13	11·68
Tin	100	51·98	....	22·80	12·76
Iron	100	52·67	25·50	....	13·24
Lead	100	52·12	24·54	21·36	....
Means	100	52·19	25·26	22·27	12·37

The lengths of the several wires, as deduced from the different comparisons, agree as nearly with each other as could possibly be expected, except those of the lead, and for any want of agreement in these I have already assigned the cause.

With the finer metals, I was under the necessity of employing shorter lengths than fifty inches on each side, for the wire of the metal having the greater intensity; but in all cases I made these as long as the length of the wire would admit. The results of the comparisons which I made with wires of copper, gold, silver, and platinum, are contained in the following Table

XI.

Metals taken as Standards.	Metals compared, and the lengths of their wires corresponding to the invariable length of the wire taken as a Standard.			
	Gold.	Silver.	Copper.	Platinum.
	inches.	inches.	inches.	inches.
Gold	36	48·82	32·7	8·0
Silver	....	70	46	11·1
Copper	....	....	100	24·76

These reduced to the standard of one hundred inches of copper wire are contained in the following Table.

XII.

Metals with which others are compared.	Metals compared, and the lengths of their wires reduced to the Standard of 100 inches of copper wire.			
	Copper.	Silver.	Gold.	Platinum.
	inches.	inches.	inches.	inches.
Copper	100	152·2	110·1	24·76
Silver...............	100	....	111·33	24·22
Gold	100	150·97	....	24·52
Platinum	100	152·74	110·34	....
Means	100	151·97	110·59	24·50

The numbers in this and Table X. represent the lengths of the wires of the respective metals, in which electricity would be excited of the same intensity as that excited in a copper wire of the same diameter, and one hundred inches long, from the same source. Since, then, with the same metal, the intensity of the electricity excited in a wire of a given diameter varies inversely as the length of the wire, the intensities in wires of the same length, but of different metals, will be directly proportional to the lengths of the wires in which the intensities are the same. These numbers will, consequently, represent the relative intensities in wires of the same length of the different metals. If the principle, that the conducting powers of different metals are proportional to the intensities of the electricity excited in equal wires, which I have shown to be true for copper and iron, be admitted for all, then these numbers will also represent the relative conducting powers of the different metals; and it will follow, that the conducting powers of different metals will vary as the length of the wires of the same diameter which transmit electricity of the same intensity, or as the quantity of electricity transmitted through the same length of wire, from a common source, either of which may be taken as the measure of the conducting power.

The numbers which I have thus determined as representing the intensity of magneto-electricity in different metals, are not only not the same as have been previously determined for the intensities of either electricity from the ordinary machine, volta-electricity, or thermo-electricity, but the order of the metals differs from them all in some particulars. In order to form a comparison, I have placed these results and different determinations of the relative intensities of electricity in different metals, in the same Table. In Table XIII. copper is taken as the standard, and silver in Table XIV.

XIII.

Electricity from the Magnet.		Electricity from the Voltaic Battery.				Thermo-Electricity.		Electricity from the Machine.	
		Davy[*].		M. Becquerel[†].		Professor Cumming[‡].		Mr. Harris[§].	
Metals.	Intensities.	Metals.	Conducting powers[‖].	Metals.	Conducting powers.	Metals.	Conducting powers.	Metals.	Conducting powers.
Silver ..	152·0	Silver ..	109	Copper	100	Silver ..	176·5	Copper	100
Gold ..	110·6	Copper	100	Gold ..	93·6	Copper	100	Silver ..	100
Copper	100	Gold ..	73	Silver ..	73·6	Zinc ..	53	Gold ..	66·7
Zinc ..	52·2	Lead ..	69	Zinc ..	28·5	Gold ..	35·2	Zinc ..	33·3
Tin	25·3	Platinum	18	Platinum	16·4	Iron ..	24·3	Platinum	20·0
Platinum	24·5	Iron ..	14·5	Iron ..	15·8	Tin	23·9	Iron ..	20·0
Iron ..	22·3			Tin	15·5	Platinum	21·6	Tin	16·7
Lead ..	12·4			Lead ..	8·3	Lead ..	16·8	Lead ..	8·3

XIV.

Electricity from the Magnet.		Davy.		M. Becquerel.		Professor Cumming.		Mr. Harris.	
Metals.	Intensities.	Metals.	Conducting powers.	Metals.	Conducting powers.	Metals.	Conducting powers.	Metals.	Conducting powers.
Silver ..	100	Silver ..	100	Copper .	135·9	Silver ..	100	Copper .	100
Gold ..	72·8	Copper .	91·7	Gold ..	127·2	Copper .	56·7	Silver ..	100
Copper .	65·8	Gold ..	66·7	Silver ..	100	Zinc ..	30·0	Gold ..	66·7
Zinc ...	34·3	Lead ..	63·3	Zinc ...	38·7	Gold ..	19·9	Zinc ...	33·3
Tin	16·6	Platinum	16·7	Platinum	22·3	Iron ...	13·7	Platinum	20·0
Platinum	16·1	Iron ...	13·3	Iron ...	21·5	Tin	13·5	Iron ...	20·0
Iron ...	14·7			Tin	21·0	Platinum	12·2	Tin	16·7
Lead ...	8·2			Lead ...	11·3	Lead ...	9·5	Lead ..	8·3

In Table XIII., in which copper is the standard, the agreement is the closest between Professor Cumming's results and those which I have obtained: the intensities of zinc, iron, tin, platinum, are nearly the same, though the order of

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[*] Philosophical Transactions, 1821.

[†] Annales de Chimie, 1826.

[‡] Manual of Electro-Dynamics, p. 288.

[§] Philosophical Transactions, 1827.

[‖] Deduced from the lengths of the wires of these metals which discharged the electricity of the same battery, p. 433. If the conducting powers are estimated by the number of pairs of plates discharged by the same length of wire of each, they appear to be

Silver .	116	Or taking silver as the standard	100
Copper .	100		86
Tin . .	21·4		18·5
Platinum .	19·8		17·0
Iron . .	16·1		14·0
Lead . .	82		71

the last three is different; and the intensities of silver do not very greatly differ in the two cases.

In Table XIV., where silver is taken as the standard, some striking coincidences may be noticed. The intensities of zinc, tin, lead, are very nearly the same as the conducting powers of those metals determined by Mr. Harris, and the difference with gold is not great. The intensity of platinum is nearly the same as the conducting power determined by Davy; those of iron also agree pretty nearly, and those of gold do not greatly differ. That these coincidences are not observable when copper is taken as the standard, may perhaps be partly attributable to impurities usually found in the copper of commerce, of which the wire I employed consisted. Mr. Harris states that the metals which he employed were pure; and I presume that the same was the case with those made use of by Sir Humphry Davy, though he does not state so. In neither of the Tables are similar coincidences observable between M. Becquerel's results and those which I have obtained, or indeed any of the others, excepting that his determinations for platinum and iron do not differ much from Mr. Harris's results.

It is to be expected, that, from whatever source electricity may be derived, whether from the voltaic battery or the magnet, whether it be excited by heat or by friction, all other circumstances being alike, the relative intensities with which it is excited in different metals will be the same; and if so, all the determinations in the Tables ought to have agreed. It is not for me to put my own observations, or the methods I have adopted, in competition with those of others, but I think that circumstances may have interfered in other methods of obtaining the intensity of electricity, which could have no influence on the results obtained with the magnet by the method I adopted.

Taking the intensity of the electricity discharged as a measure of the conducting power, and assuming that this intensity varies as the number of plates in the battery,—a principle, however, which must be established independently,—the method adopted by Sir Humphry Davy would determine the conducting powers of metals. But there are many circumstances that would interfere with the accuracy of the results, some of which are noticed by the author, and others are pointed out by M. Becquerel; and these or others may not have equally affected all the metals. That some circumstance must have particularly affected the results obtained with lead, and that to a considerable extent, is quite evident. Taking silver as the standard, 100, the conducting power of lead, determined by the length of the wire discharging the electricity of the same number of plates, is 63; and by the number of pairs of plates it is 71: whereas, taking the same standard, it is only 11·3 according to M. BECQUEREL, 9·5 according to Professor CUMMING, 8·3 according to Mr. HARRIS, and 8·2 according to the experiments I have detailed.

In the arrangement adopted by M. BECQUEREL, two currents, in separate wires, and in contrary directions, influence the galvanometer needle; and these currents are rendered equal by shortening the wires of one until the needle retains its original position; two secondary circuits being then formed, it is assumed that the intensity of the electricity in these will be equal, if the needle remains uninfluenced. It is, however, not quite clear that this will be the case, if the want of symmetry in the arrangement of the wires of the galvanometer is compensated by a difference in their lengths*. The wires made use of by M. BECQUEREL were short, not more than four or five inches in length, so that a small error in their lengths would cause a considerable difference in their comparative conducting powers, but not sufficient to account for the differences between his results and mine.

In comparing the electric powers of different metals by means of a thermo-electric apparatus, great precaution is necessary to guard against any anomalous influence which the agent in this case employed for exciting electricity may exert upon different metals. It is possibly to such influence that we are to attribute the very low electric power of gold determined by Professor CUMMING. This is indeed the only marked instance of disagreement between his results and those which I obtained.

The principle employed by Mr. HARRIS, and first suggested by Mr. CHILDREN,—namely, that the heat evolved by the passage of electricity through conductors is in some inverse ratio of the conducting power,—was employed by Sir

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* When first I made use of the arrangement which I have described, the subject being quite new to me, I was not aware of that employed by M. BECQUEREL. There is some similarity in the two, but the principles on which their application depends are very different. M. BECQUEREL's depends upon two equal currents, in separate wires, being equally diminished by two other currents, likewise in separate wires: mine, on the effect of a current in a single wire being counteracted by an equal and opposite current in the same wire, or that the opposite electricities neutralize each other, so that no current exists in the wire of the galvanometer. It appears to me that my arrangement combines the advantages of greater simplicity and greater accuracy.

Humphry Davy*; and though he obtained no very definite numerical results, yet it appeared, in one instance, that the generation of heat was nearly inversely as the conducting power. According to this principle, the order of the metals relative to their conducting power was found to be silver, copper, lead, gold, zinc, tin, platinum, palladium, iron, which corresponds very nearly with the order previously determined by the lengths of their wires, and also, with the exception of lead and gold, with the results I have obtained. That the principle may give in some cases a close approximation to the truth, I think is probable from the agreement of the results obtained by means of its application; but I think it more likely that the conducting power is a function both of the absolute heat and of the specific heat of the conductor. I have already noticed the agreement between Mr. Harris's results and mine; possibly the differences observable in the two cases are to be attributed to this circumstance; and also that the heat evolved may not be a measure of the heat of the conductor.

I am very desirous of obtaining results with the voltaic battery, and also if possible with a thermo-electric apparatus, and the common machine, by means of an arrangement similar to that which I have employed with the magnet, as I consider that a comparison of results obtained by the same means in the different cases must throw some additional light upon this interesting subject; and I shall avail myself of the first opportunity of leisure which I have to prosecute the inquiry.

Royal Military Academy.
8th January 1833.

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* Philosophical Transactions, 1821, p. 438. In this valuable communication the author states, that the most remarkable general result which he obtained was, that "the conducting power of metallic bodies varied with the temperature, and was lower in some inverse ratio as the temperature was higher:" and that whether the heat was occasioned by the electricity, or applied from some other source, the effect was the same. The conducting power being as the intensity, it would follow that the intensity varies in some inverse ratio of the heat of the conductor. On this point I have not had leisure to make any experiments with a view to obtain numerical results, but I have found that a diminution of intensity is the consequence of an increase of temperature. Four copper wires of precisely the same length, each thirty inches, were connected so that the currents transmitted through them to the galvanometer neutralized each other: heat was then applied to the two which crossed each other, and through which one of the currents was transmitted. On the contact of the iron cylinder with the magnet being made, the needle deviated 9° in one direction, and on its being broken, it deviated 8° in the other; and these directions indicated a diminished intensity in the heated wires.

X. *Notice of the remains of the recent Volcano in the Mediterranean. By* JOHN DAVY, *M.D. F.R.S. Assistant Inspector of Army Hospitals.*

Read March 8, 1833.

IN consequence of an examination made by Captain SWINBURNE, R.N., of the shoal formed by the subsidence of the volcanic island which was thrown up in the summer of last year, I am enabled to give a few more particulars respecting it, which I beg leave to communicate to the Royal Society, not so much on account of the importance of the particulars themselves, as for the sake of endeavouring to contribute as much as possible towards the history of the volcano.

I shall insert an extract from a letter of Captain SWINBURNE, containing the results of his examination, dated the 24th of August, addressed by him to the admiral on the station, and which was published in the Malta Gazette of the same date.

"It" (the volcano) "has left a dangerous shoal, consisting principally of black sand and stones, with a circular patch of rock in the middle of it, about forty-two yards in diameter, on which there are two fathoms water generally, but in one spot only nine feet. All round the rock there are from two and a half to three fathoms, deepening gradually to five and six fathoms at the average distance of one hundred yards from the centre, then more rapidly to ten, twenty, thirty, forty, &c. fathoms.

"A small detached rock with fifteen feet on it lies one hundred and thirty yards to the S.W. of the central patch. About three quarters of a mile N.W. of the centre there is a detached bank with twenty-three fathoms on it. All the rock appears to be dark-coloured porous lava, and the sand (which is extremely fine in the deepest water,) is composed entirely of particles of the same substance. By this the soundings near the shoal may be distinguished; but it should be approached with great caution, as a large extent of discoloured but deep water which lies to the S.W. may be mistaken for it, while the real danger is seldom visible till it is near, being composed of very dark-coloured materials, and it is so deep that the lead cannot be trusted. Its latitude and longitude, as far as my limited means of observation enable me to decide, are 37° 9′ N. and 12° 43′ E. of Greenwich."

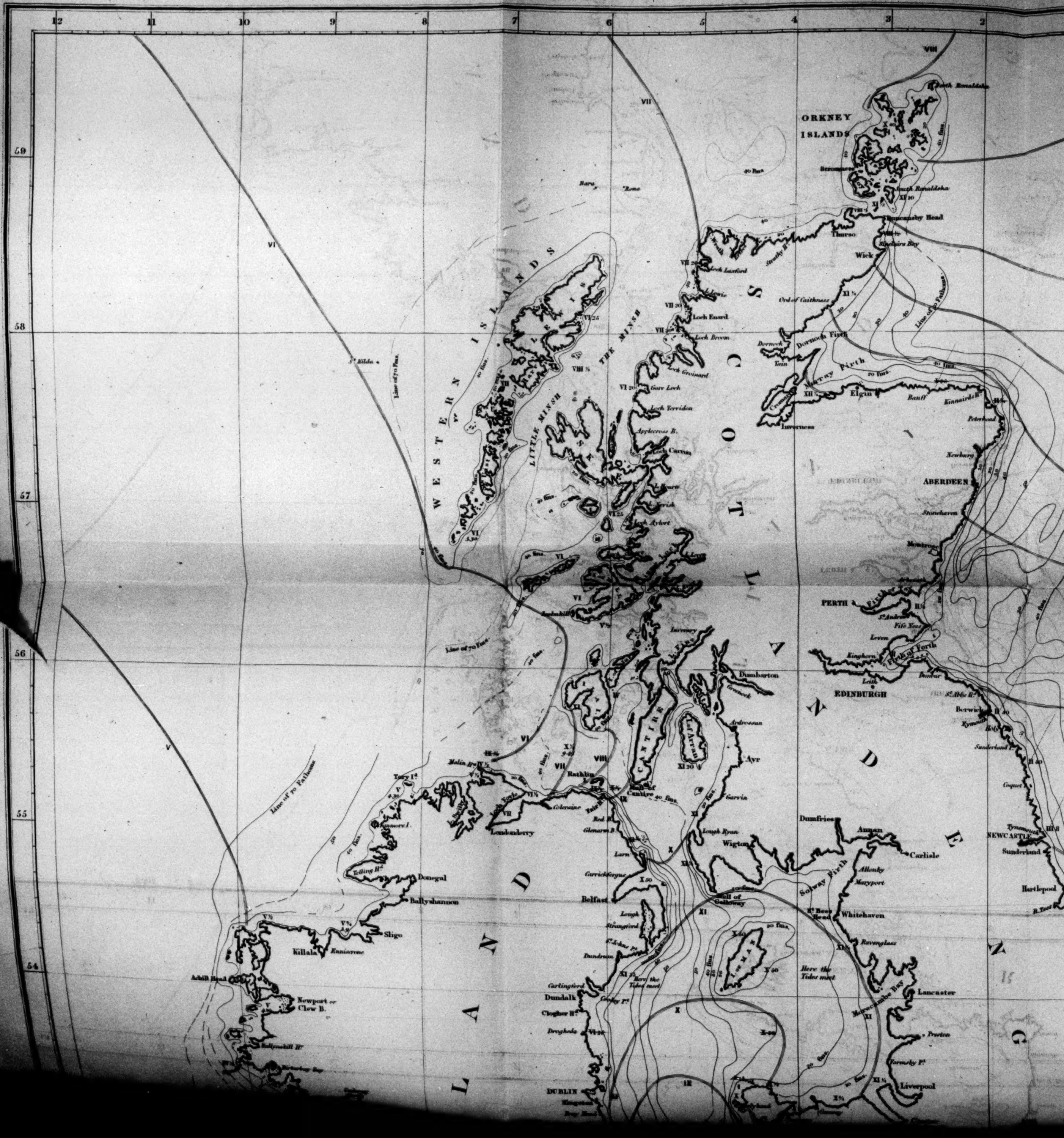
In conversation with Captain SWINBURNE previous to his proceeding to examine the shoal, I mentioned to him as a desideratum, the collecting a portion of any air which might be seen rising from the site of the volcano; and, on his return into port on the 22nd of August, he obliged me with two specimens which he had procured. They were contained in wine-bottles, which were corked and inverted in water. Each bottle was about half-full of air. According to Captain SWINBURNE's statement, the air was collected and preserved with all possible care, so as to preclude the possibility of any admixture of external atmospheric air. In a letter with which this gentleman has favoured me on the subject, after describing how the air was collected and preserved, he adds: "It came up in many places in small silver threads of bubbles; the quantity varied from time to time in the same place, and where it was most plentiful, the bottom, looking at it through a water-glass, had somewhat the appearance that is observed in chalybeate springs; the cinders (elsewhere quite black) had a rusty appearance. I could detect no difference in the temperature of the water at these spots, nor any between the water on or near the shoal and that at a distance from it."

On the 23rd of August, about twelve hours after I had received the bottles of air, I examined them. On withdrawing the corks under water, in neither bottle was there any diminution of the volume of the air;—no indication of any absorption having been produced by the salt water with which it had been in contact for several days. Though collected at some distance from each other, I found both specimens of air very nearly similar. Neither of them was inflammable, nor had any smell of sulphuretted hydrogen, nor was absolvable by lime water, and both extinguished flame. Forty-one measures of one, by phosphorus were diminished to 37, and thirty-five measures of the other were diminished to 31·5; indicating the presence of between nine and ten parts of oxygen, mixed with between seventy-nine and eighty of azote.

The origin of this air must necessarily be matter of speculation. Two views may be taken of it. It may either be supposed to have arisen from the extinct volcano; or, to have been disengaged from sea water at the bottom in contact with, and penetrating into the loose and probably hot ashes and cinders composing the shoal. The situation of the volcanic shoal so far from land, and all the circumstances best authenticated relative to the volcano when in activity, seem very unfavourable to the first supposition. The second supposition seems more reasonable. Had the air consisted of oxygen and azote in the same proportions as exist in the atmosphere, it would hardly be a subject for question. The presence of about as much as ten per cent. oxygen is strong proof that the source of the air was not very deep, not beneath the bed of the sea. Supposing it disengaged from the water, as alluded to, it is easy to conceive how it might be deprived of its oxygen. The oxygen of atmospheric air absorbed by sea water probably becomes less and less in quantity with the depth to which it penetrates, both from the action of living and dead matter swimming and suspended in the water, so that in a very deep sea not a particle of oxygen may reach the bottom. In addition to which general causes for the abstraction of oxygen, in the instance of this shoal there appears to have been a special one in operation; viz. the black oxide of iron entering into the composition of the cinders and ashes derived from the volcano. Now, as Captain Swinburne expressly states, that where the air rose, there "the cinders had a rusty appearance," indicating the conversion of the black oxide into red, it clearly follows that the change must have been owing to an absorption of oxygen; and it can hardly be doubted that it was derived from the air in question. I may add, that the minute quantity in which the air was disengaged, is favourable to the second supposition rather than the first. Rising "in silver threads of bubbles," as described by Captain Swinburne, is what might be expected on the idea of its being disengaged by the water. Had it been evolved in large volumes, then this source would have been inadequate, and it would have been necessary to have sought for it deeper, in connexion with, or as a product of the volcano. To obviate a possible objection, I would further beg to add, that the temperature of the water at the surface, where the gas was ascending, being the same as in the adjoining sea, is no proof that at the bottom it was not higher. What I witnessed in relation to the temperature of the sea close to the volcano when it was in activity, as described in my first communication to the Society, is sufficiently demonstrative of this.

The explanation just proposed of the origin of a mixture of oxygen and azote in this instance, may probably admit of wide extension, and may be applicable to all hot springs from which azote is disengaged, whether pure, as in a few examples, or mixed with other gases, as is more common. The water feeding the springs, derived from the atmosphere, on entering the ground, must contain a certain quantity of common air; the oxygen of which, from a variety of substances attracting it, may be separated and absorbed in the descent and re-ascent of the water through the strata of the earth, whilst the azote will remain free to be disengaged from elevation of temperature, or removal of pressure when the spring bursts forth at the surface.

Malta,
November 15th, 1832.



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Inverness
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Banff
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Solway Firth
Allonby
Maryport
St. Bees Head
Whitehaven
Ravenglass
Lancaster
Morecambe Bay
Preston
Formsby Pt.
Liverpool
Here the Tides meet
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Wigton
Lough Ryan
Garvin
Ayr
Ardrossan
Isle of Arran
CANTIRE
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Inverary
Rathlin
Fair Hd.
Coleraine
Londonderry
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Aranmore I.
Telling Hd.
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Ballyshannon
Sligo
Killala
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Newport or Clew B.
Ballynakill Hr.
Red B.
Glenarm B.
Larn
Carrickfergus
Belfast
Lough Strangford
St. Johns Pt.
Dundrum
Carlingford
Dundalk
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Gare Loch
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Applecross B.
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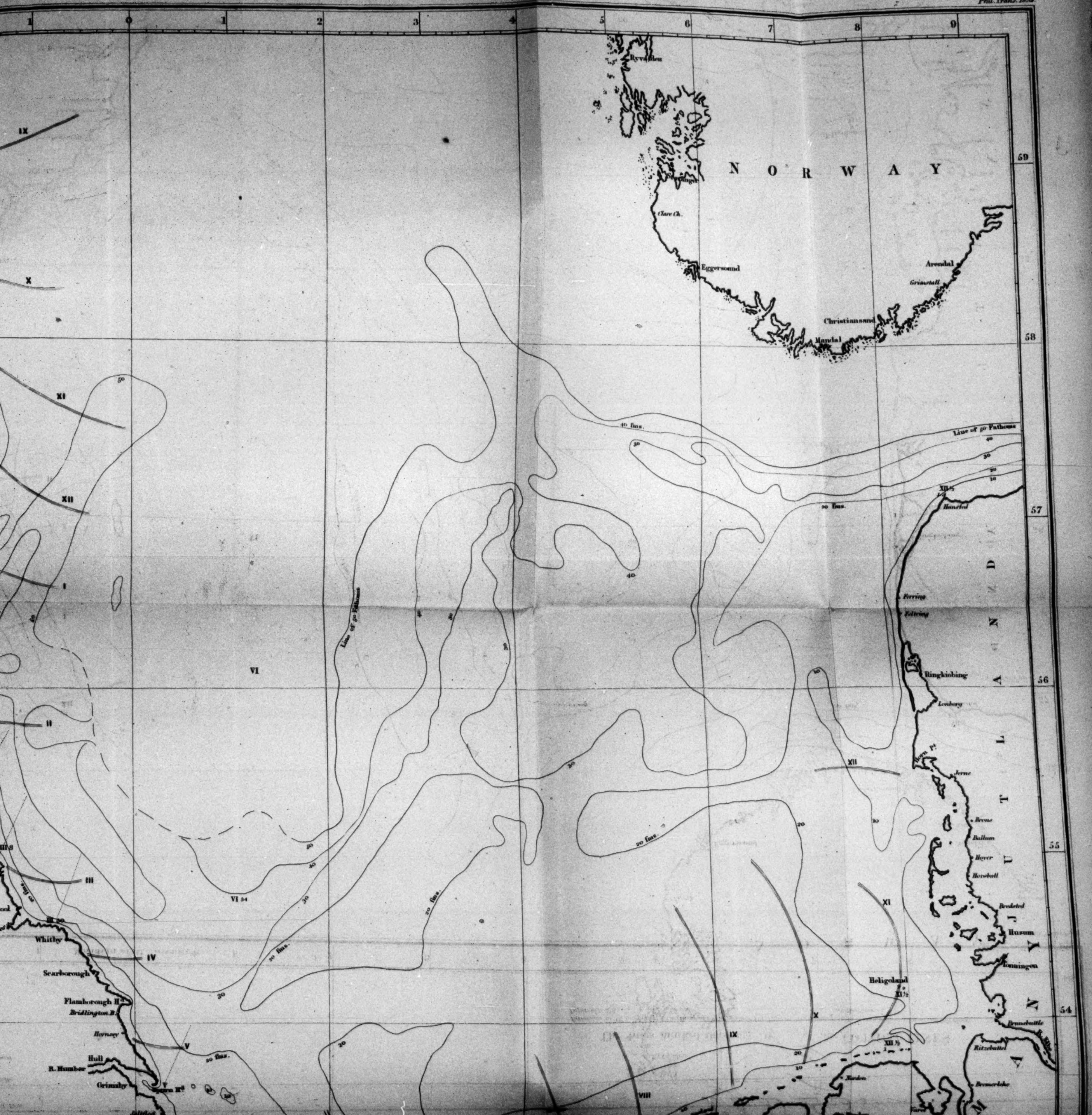
Phil. Trans. 1833.



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Bredsted
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Brunsbuttle
R. Elbe
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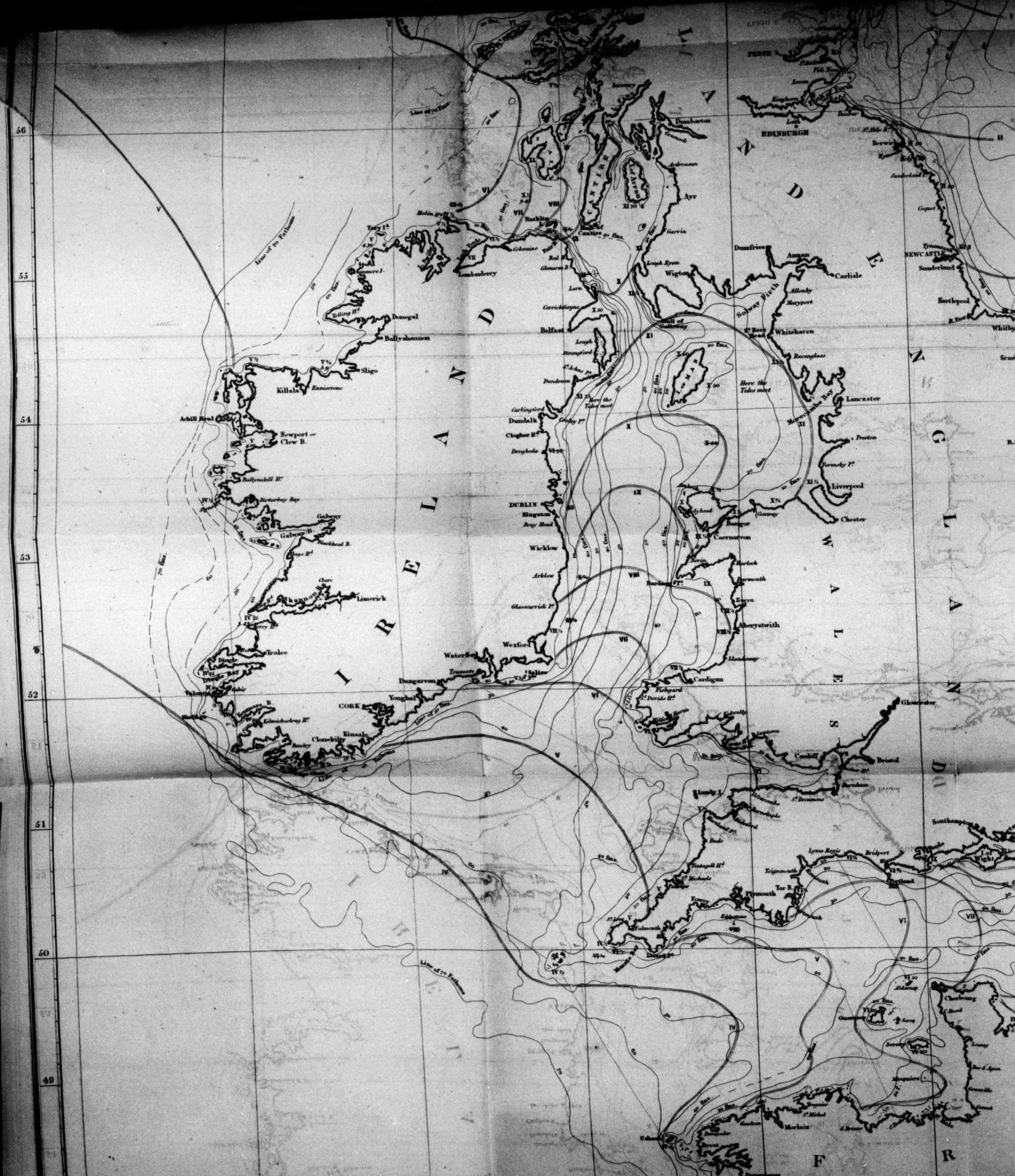

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Coleraine
Rathlin
Cantire
Telling Hd.
Donegal
Ballyshannon
Sligo
Killala
Enniscrone
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Ballynakill Hd.
Birterbuy Bay
Galway
Galway B.
Black head B.
Loop Hd.
Clare
Shannon R.
Limerick
Kerry Hd.
Tralee
Dingle
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Valentia
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Dungarvon
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Waterford
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Mull of Galloway
Dumfries
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Allonby
Maryport
Whitehaven
St. Bees Head
Ravenglass
Morecambe Bay
Lancaster
Preston
Formsby Pt.
Liverpool
Chester
Conway
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Barmouth
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Cardigan
Fishgard
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Milford Haven
Gloucester
Bristol
Cardiff
Burnham
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Barnstaple
Hartland
Bude
Tintagell Hd.
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Padstow
St. Ives
Falmouth
Lizard Pt.
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Eddystone
Tor B.
Teignmouth
Lyme Regis
Bridport
Portland
Southampton
I. of Wight
Alderney
Guernsey
Sarq
Jersey
Minquiers
Cherbourg
Granville
Morlaix
St. Michel
Treguier
Ushant
Douarnenez Bay
EDINBURGH
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Dunbar
Berwick
Sunderland
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Whitby
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CANTIRE
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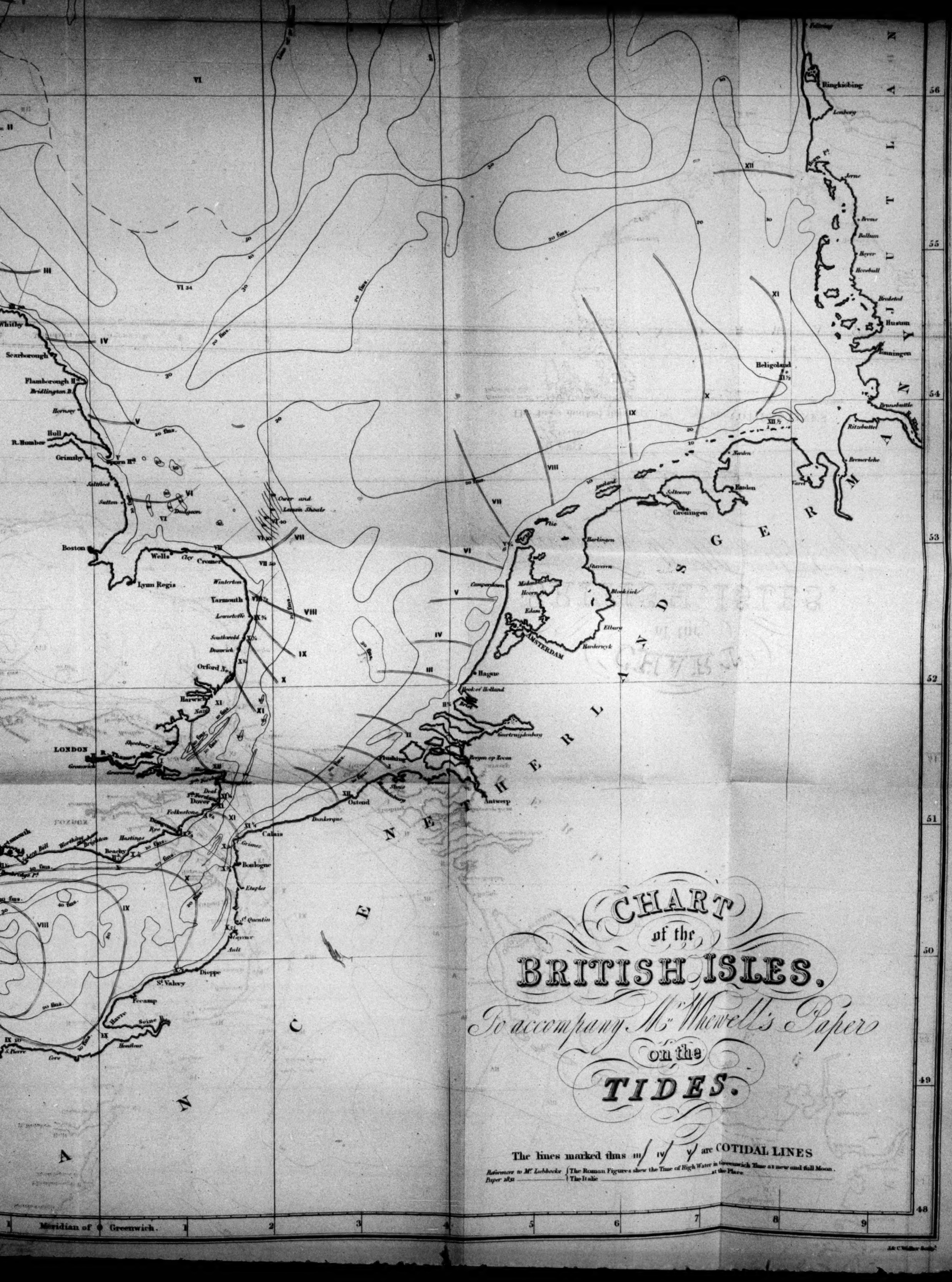
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Groningen
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Staveren
Blokziel
Elburg
Harderwyk
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Hoorn
Edam
AMSTERDAM
Camperdown
Hague
Hook of Holland
Geertruijdenberg
Bergen op Zoom
Antwerp
Flushing
Sluis
Ostend
Dunkerque
Calais
Grisnez
Boulogne
Etaples
St. Quentin
Cayeux
Ault
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St. Valery
Fecamp
Havre
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Grimsby
Spurn Hd.
Saltfleet
Sutton
Dudgeon
Owner and Leman Shoals
Boston
Lynn Regis
Wells
Cley
Cromer
Winterton
Yarmouth
Lowestoffe
Southwold
Dunwich
Orford Ness
Harwich
Naze
LONDON
Greenwich
Gravesend
Deal
Dover
Folkestone
Rye
Hastings
Beachy Hd.
Brighton
Worthing
JUTLAND
GERMANY
NETHERLANDS
FRANCE
20 fms.
30
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I
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III
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V
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XI
XII
56
55
54
53
52
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50
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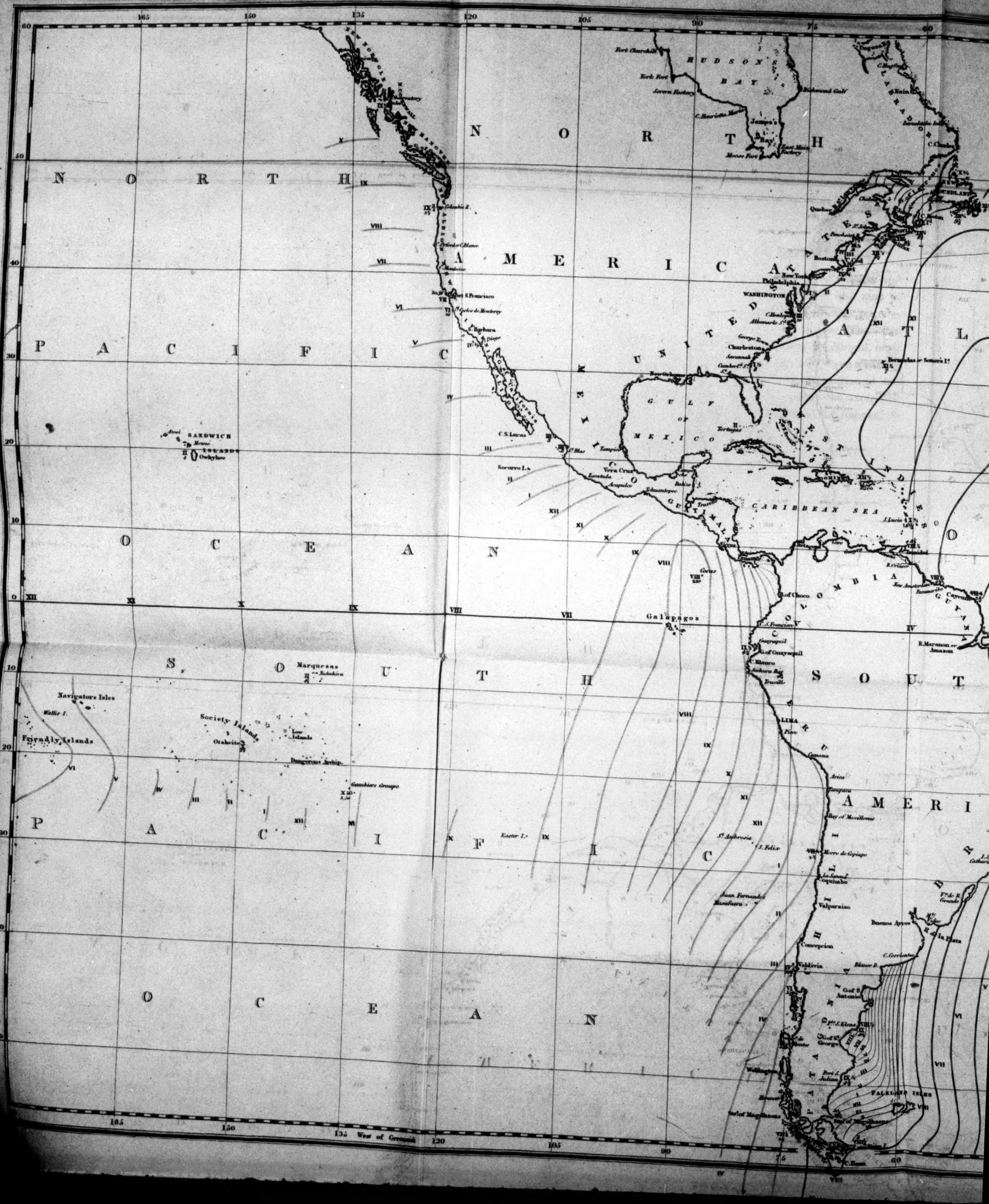
48
1
Meridian of Greenwich
1
2
3
4
5
6
7
8
9
Jas. C. Walker Sculp.

CHART
of the
BRITISH ISLES,
To accompany Mr. Whewell's Paper
on the
TIDES.

The lines marked thus III/ IV/ V/ are COTIDAL LINES

References to Mr. Lubbocks Paper 1831	The Roman Figures shew the Time of High Water in Greenwich Time at new and full Moon. The Italic ―――― at the Place



NORTH PACIFIC OCEAN
SOUTH PACIFIC OCEAN
NORTH AMERICA
UNITED STATES
MEXICO
GUATIMALA
COLOMBIA
PERU
CHILI
SOUTH AMERICA
GULF OF MEXICO
CARIBBEAN SEA
HUDSON BAY
James's Bay
WEST INDIES
Fort Churchill
York Fort
Severn Factory
Moose Fort
East Main Factory
Richmond Gulf
C. Henrietta Maria
Quebec
Boston
New York
Philadelphia
WASHINGTON
C. Henlopen
Albemarle Sd.
Charleston
Savannah
New Orleans
Tortugas
Bermudas or Somers Is.
Columbia R.
C. Orford or C. Blanco
C. Mendocino
Pt. S. Francisco
Pt. Carlos de Monterey
S. Barbara
S. Diego
C. S. Lucas
S. Blas
Tampico
Vera Cruz
Acapulco
Tehuantepec
Socorro I.
Balize
Truxillo
Caribbean Sea
S. Lucia
Trinidad
R. Orinoco
New Amsterdam
Paramaribo
Cayenne
R. Maranon or Amazon
G. of Choco
C. S. Francisco
Galapagos
Cocos
Guayaquil
G. of Guayaquil
C. Blanco
Truxillo
LIMA
Pisco
Camana
Arica
Iquique
Bay of Mexillones
Morro de Copiapo
Coquimbo
Valparaiso
S. Ambrosio
S. Felix
Juan Fernandez
Masafuera
Concepcion
Valdivia
Buenos Ayres
R. de la Plata
C. Corrientes
Blanco B.
G. of S. Antonio
C. S. Helena
G. of S. George
Port S. Julian
FALKLAND ISLES
Str. of Magalhaens
C. Horn
Wellington
SANDWICH ISLANDS
Owhyhee
Atooi
Mowee
Marquesas
Nukahiva
Navigators Isles
Wallis I.
Friendly Islands
Society Islands
Otaheite
Low Islands
Dangerous Archip.
Gambiers Groupe
Easter I.
NEW NORFOLK
NEW HANOVER
NEW ALBION
CALIFORNIA
LABRADOR
Nain
165
150
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120
105
90
75
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West of Greenwich
60
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I
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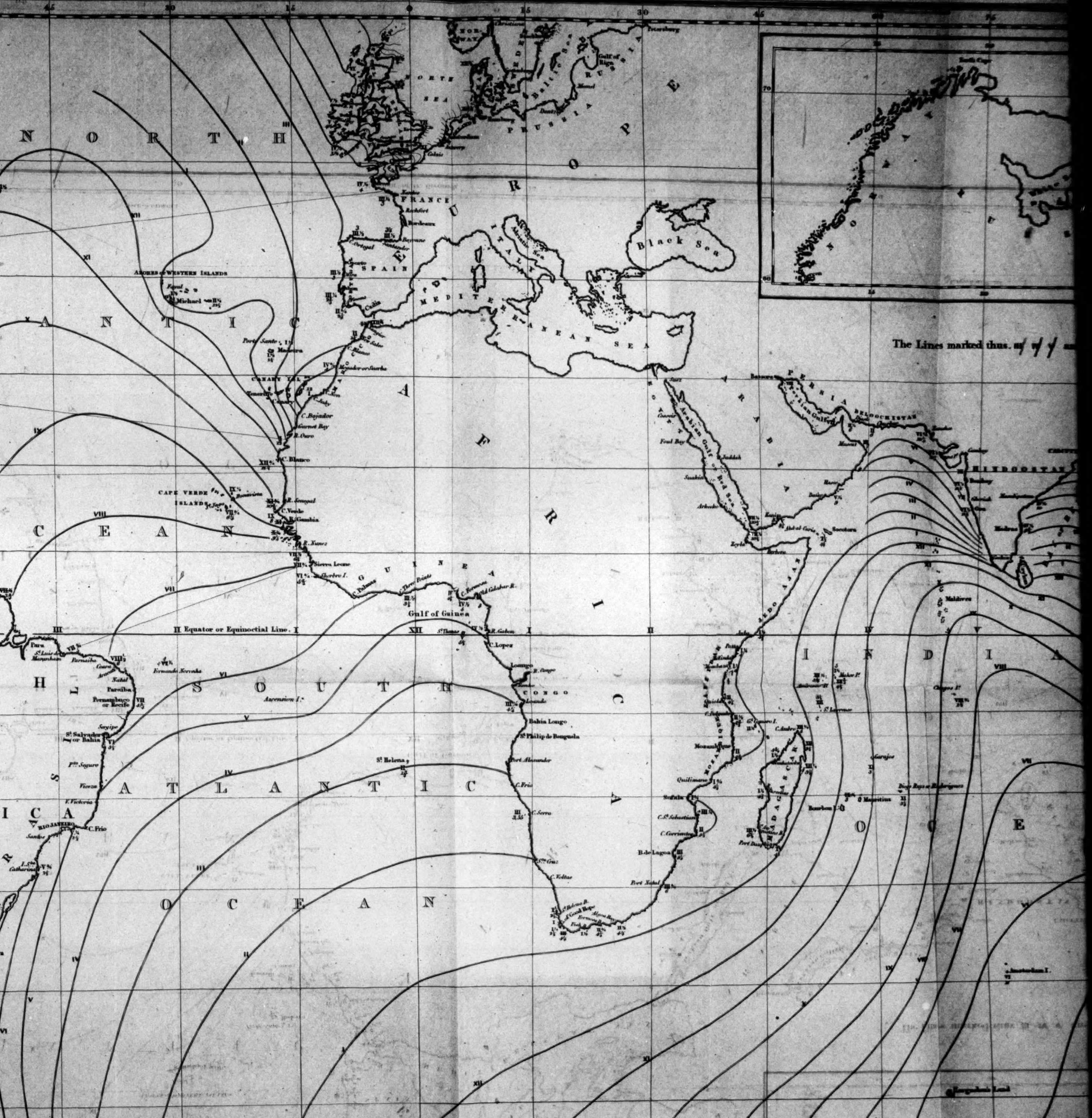
N O R T H
A T L A N T I C
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I N D I A
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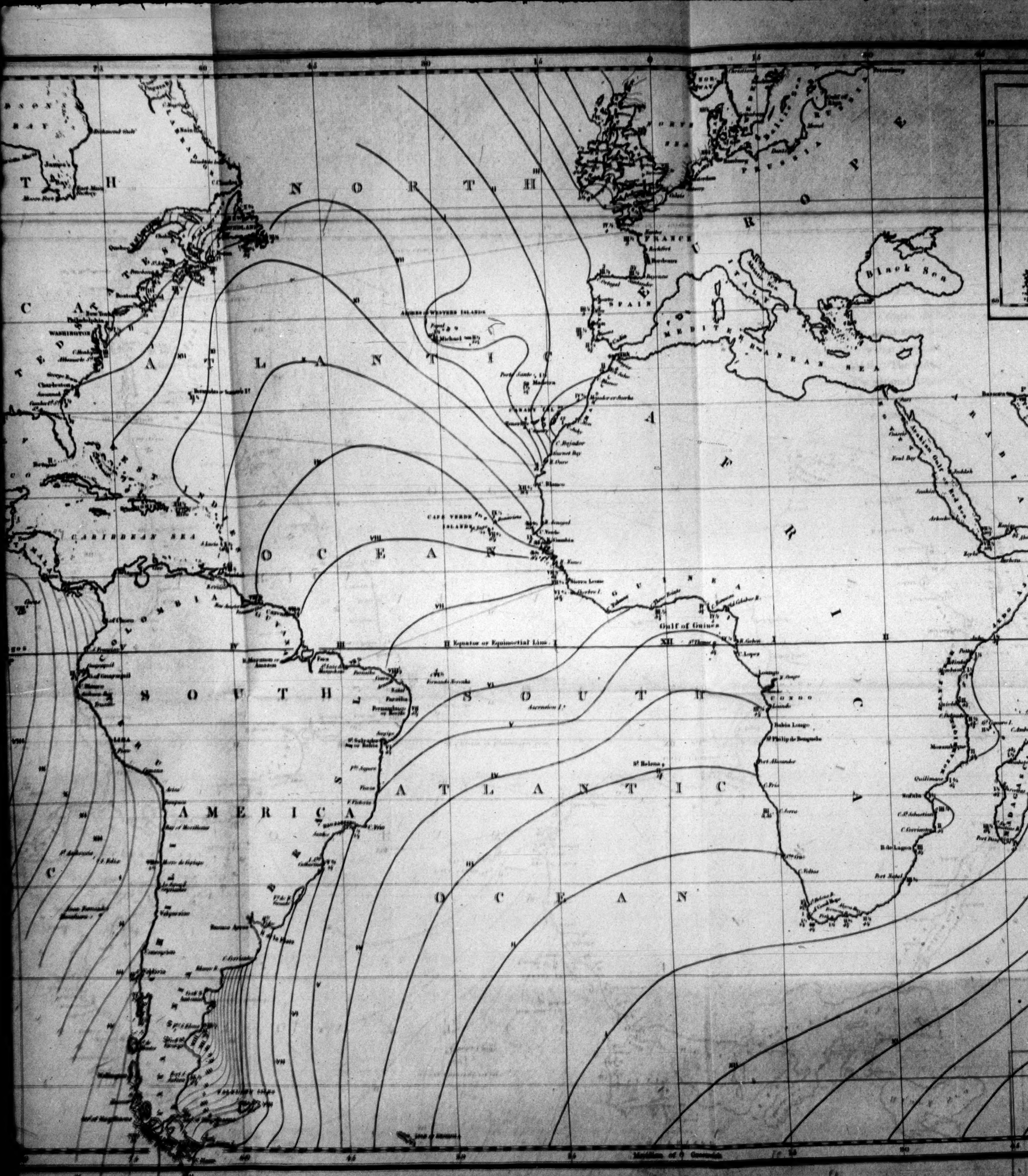
NORTH SEA
FRANCE
SPAIN
ITALY
PRUSSIA
RUSSIA
Baltic Sea
Gulf of Riga
Petersburg
Black Sea
MEDITERRANEAN SEA
Adriatic Sea
AZORES or WESTERN ISLANDS
Fayal
St. Michael
Porto Santo
Madeira
CANARY ISL.
Teneriffe
C. Bojador
Garnet Bay
R. Ouro
C. Blanco
CAPE VERDE ISLANDS
Bonavista
R. Senegal
C. Verde
R. Gambia
R. Nunez
Sierra Leone
Sherbro I.
C. Palmas
C. Three Points
GUINEA
Gulf of Guinea
Old Calabar R.
R. Gabon
C. Lopez
St. Thomas
Equator or Equinoctial Line.
Loango
R. Congo
CONGO
Loando
Bahia Longo
St. Philip de Benguela
Port Alexander
C. Frio
C. Serra
St. Helena
Ascension I.
St. Cruz
C. Voltas
St. Helena B.
C. of Good Hope
Algoa Bay
Port Natal
B. de Lagoa
C. Corrientes
C. St. Sebastian
Sofala
Quilimane
Mozambique
Comoro I.
C. Ambre
MADAGASCAR
Port Dauphin
Bourbon I.
Mauritius
Diego Rays or Rodrigues
Seychelles
Amirantes
St. Lawrence
Garajos
Chagos Is.
Maldives
ARABIA
PERSIA
BELOOCHISTAN
Persian Gulf
Arabian Gulf or Red Sea
Suez
Cosseir
Foul Bay
Juddah
Suakin
Arkeeko
Mocha
Aden
Zeyla
Berbera
Socotora
Muscat
Bassora
Bombay
Goa
HINDOOSTAN
Madras
Amsterdam I.
Kerguelen's Land
ISLE of GEORGIA
Para
St. Luis de Maranham
Parnaiba
Ceara
Natal
Paraiba
Pernambuco or Recife
Fernando Noronha
Sergipe
St. Salvador or Bahia
Pto. Seguro
Vitoria
RIO JANEIRO
C. Frio
Santos
I. St. Catharina
AMERICA
South Cape
NORWAY
The Lines marked thus



NORTH ATLANTIC OCEAN
SOUTH ATLANTIC OCEAN
SOUTH AMERICA
AFRICA
EUROPE
ARABIA
COLOMBIA
GUINEA
Gulf of Guinea
CONGO
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Fernando Noronha
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C. Frio
Buenos Ayres
Rio la Plata
FALKLAND ISLANDS
Valparaiso
Lima
Guayaquil
Strait of Magellan
C. Horn
Benguela
Port Alexander
C. Frio
C. Voltas
Port Natal
Delagoa Bay
Sofala
Quilimane
Mozambique
Meridian of Greenwich
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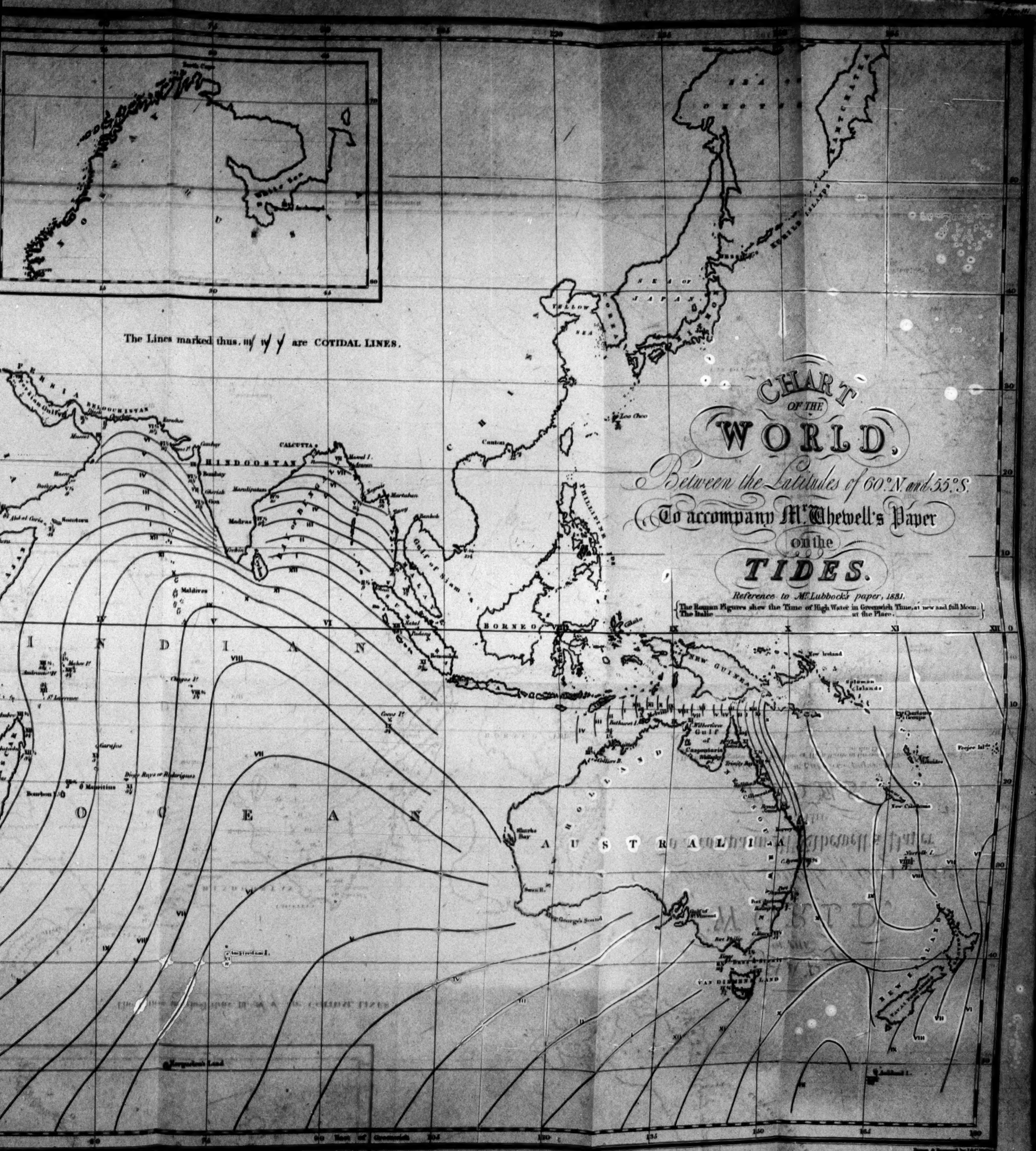


CHART OF THE WORLD,
Between the Latitudes of 60°. N. and 55°. S.
To accompany Mr. Whewell's Paper on the TIDES.
Reference to Mr. Lubbock's paper, 1831.
The Roman Figures shew the Time of High Water in Greenwich Time, at new and full Moon.
The Italic at the Place.
The Lines marked thus are COTIDAL LINES.
INDIAN OCEAN
AUSTRALIA
HOLLAND
HINDOOSTAN
CALCUTTA
Bombay
Madras
Maldives
Chagos I.
Cocos I.
Mauritius
Bourbon
Diego Rays or Rodrigues
Amsterdam I.
Kerguelen's Land
BORNEO
NEW GUINEA
New Ireland
Solomon Islands
New Caledonia
Norfolk I.
NEW ZEALAND
VAN DIEMEN'S LAND
Gulf of Carpentaria
Sharks Bay
King George's Sound
Bass's Straits
Gulf of Siam
Canton
Loo Choo
SEA OF JAPAN
YELLOW SEA
SEA OF OKHOTSK
KAMTSCHATKA
KURILE ISLANDS
PHILIPPINE
Feejee Isl.
Auckland I.
IX
X
XI
XII
60 East of Greenwich
75
90
105
120
135
150
165
180

XI. *Essay towards a First Approximation to a Map of Cotidal Lines. By the Rev.* W. Whewell, *M.A. F.R.S. Fellow of Trinity College, Cambridge.*

Read May 2, 1833.

Introduction.

EVER since the time of Newton, his explanation of the general phenomena of the tides by means of the action of the moon and the sun has been assented to by all philosophers who have given their attention to the subject. But even up to the present day this general explanation has not been pursued into its results in detail, so as to show its bearing on the special phenomena of particular places,—to connect the actual tides of all the different parts of the world,—and to account for their varieties and seeming anomalies. With regard to this alone, of all the consequences of the law of universal gravitation, the task of bringing the developed theory into comparison with multiplied and extensive observations is still incomplete; we might almost say, is still to be begun.

Daniel Bernoulli, in his Prize Dissertation of 1740, deduced from the Newtonian theory certain methods for the construction of tide tables, which agree with the methods still commonly used. More recently Laplace turned his attention to this subject; and by treating the tides as a problem of the oscillations rather than of the equilibrium of fluids, undoubtedly introduced the correct view of the real operation of the forces; but it does not appear that in this way he has obtained any consequences to which Newton's mode of considering the subject did not lead with equal certainty and greater simplicity; moreover by confounding, in the course of his calculations, the quantities which he designates by λ and λ', the epochs of the solar and lunar tide (Méc. Cél. vol. ii. p. 232. 291.), he has thrown an obscurity on the most important differences of the tides of different places, as Mr. Lubbock has pointed out.

Laplace also compared with the theory observations made at Brest from the year 1711 to 1715; and showed that the laws which, according to the theory, ought to regulate the times and heights of the tides, may, in reality, be traced in the averages of this series of observations. In pursuance of his advice also, a new series of observations was undertaken at the same port, with the intention that it should be continued, at least, during one period of the motion of the nodes of the moon's orbit. The new observations were begun in 1806, and have since been carried on without interruption. Of the observations thus made, LAPLACE subjected to a mathematical discussion those for sixteen years, beginning with 1807; and M. BOUVARD, who performed the requisite calculations, employed nearly 6000 tide-observations. In our own country also, Mr. LUBBOCK has given the results of the examination of about 13,000 tide-observations, made at the London Docks, from 1808 to 1826, in a Memoir recently published in these Transactions. These results are very important, in consequence of their consistency with theory and with each other; the calculations by which they were obtained were performed by Mr. DESSIOU; and the task which he has thus executed, is, perhaps, in the amount of labour, and in the judicious and systematic mode of its application, not inferior to any of the most remarkable discussions of large masses of astronomical or meteorological observations by other modern calculators.

But in the meantime no one appears to have attempted to trace the nature of the connexion among the tides of different parts of the world. We are, perhaps, not even yet able to answer decisively the inquiry which BACON suggests to the philosophers of his time, whether the high water extends across the Atlantic so as to affect contemporaneously the shores of America and Africa, or whether it is high on one side of this ocean, when it is low on the other; at any rate such observations have not been extended and generalized.

It will easily be understood that we may draw a line through all the adjacent parts of the ocean which have high water at the same time; for instance, at 1 o'clock on a given day. We might draw another line through all the places which have high water at 2 o'clock on the same day, and so on. Such lines may be called *cotidal* lines; and they will be the principal subject of the present essay.

It might perhaps be supposed at first that we have now considerable materials for drawing such cotidal lines upon our maps. The time of the tide has been observed and recorded over a large portion of the earth's surface, by residents or by voyagers, during the last two centuries; and we have in many works tables of the *establishment* of a long list of places. There are, however, in these statements, certain errors and imperfections, which prevent our being able as yet to determine the course of the cotidal lines with accuracy, or even to obtain with certainty a first approximation to these lines. But before we explain the defects of our observations, it will be proper to say a few words on the general properties of the cotidal lines.

The cotidal line for any hour may be considered as representing the summit or ridge of the *tide-wave* at that time; in which expression we mean, by the tide-wave, that protuberance of water upon the surface of the ocean which moves along the seas, and by its motion brings high-water and low-water to any place, at the time when the elevated and the depressed parts of the watery surface reach that place. The cotidal lines for successive hours represent the successive positions of the summit of this wave; and if we suppose a spectator, detached from the earth, to perceive the summit of the wave, he will see it travelling round the earth in the open ocean once in twenty-four hours, accompanied by another at twelve hours distance from it; and both sending branches into the narrower seas; and the manner and velocity of all these motions will be assigned by means of a map of cotidal lines.

I now proceed to endeavour to determine, first, from the laws of the motion of water, what the form of such lines may be expected to be; second, from the tide observations which we possess, what their form appears to be in reality.

Sect. I. *On Cotidal Lines as determined by the laws of fluids.*

1. *Tides on a globe covered with water.*—If we suppose the whole surface of the terrestrial globe to be uniformly covered with water, it is easy to see what must be the nature of the form and motion of the cotidal lines. The tides would be, in their main circumstances, entirely governed by the moon. High water at every place, in the same latitude, would follow the transit of the moon at the same interval of time*. The points at which it was high water at a given moment would therefore be situated in a meridian, at a certain distance from the meridian in which the moon was (or at least in some curve symmetrical with regard to the equator). There would be one such curve having reference to the moon, and another, having reference to the point

* We here consider the moon as moving in the equinoctial.

immediately opposite to the moon; and these curves would each revolve round the earth, from east to west, in something more than twenty-four hours. If we suppose one cotidal line to be drawn through all points at which it is high water at 1 o'clock on a given day, a second cotidal line through all places where it is high water at 2 o'clock on the same day, and so on, there will be twenty-four such similar lines on the whole surface of the globe, cutting the equator at equal intervals, like so many meridians. And since the circumference of the earth is about 25,000 miles, it is obvious that any one of these cotidal lines would travel with a velocity of above one thousand miles an hour at the equator, and with a velocity of about six hundred miles an hour in our latitude. This is the velocity with which the summit of the tide-wave would travel on this supposition.

2. *Derivative tides.*—If on such a globe as we have been considering, a continent were interposed, occupying a great extent of latitude, it is clear that the motion of the cotidal lines must become quite different from what it was in the uninterrupted ocean. On the western side of such a continent the tide-wave could no longer proceed as if the continent were not there; for the supply of water and of pressure brought by the tide-wave advancing from the east, on which its further motion westwards altogether depends, is entirely intercepted. The tide on the western side of the continent must be produced by the water and the pressure which comes from the north, south, and west, and will be governed by laws different from those which regulate the primary or uninterrupted tide. And the same may be said of the tides produced in any seas of which the extent is much intercepted by land.

In order to see the general character of such cases, let us take the case of a tide which is entirely derived from the primary tide, and is not affected at all by the direct action of the sun and moon. Suppose the surface of the southern hemisphere to be entirely occupied by water, and the northern hemisphere to be principally land. Let a considerable inland sea run northward from the equator towards the pole. The tide-wave of the southern ocean, as it passes the entrance of this sea, will send off a derivative undulation, which will advance northwards up the sea, being impelled entirely by the mechanical action by which undulations are propagated in fluids. If we suppose the depth and other circumstances which would affect the motion of this derivative wave to be uniform in different parts of the sea, the wave will advance in the direction in which it first sets out, and therefore, if the entrance of the sea be narrow, in the direction of the length of the sea. This wave will bring a tide wherever it arrives, and the cotidal lines so produced will be nearly perpendicular to the length of the sea. The velocity with which the wave moves will depend on various circumstances, but principally on the depth, and probably on the regularity of the channel. If the depth be nearly uniform, the cotidal lines will be nearly straight and parallel. Their rectilinear and parallel character depend on this; that the propagation of a wave may be conceived to result from the propagation of undulations in every direction from every point of the line of the wave; and the assemblage of the undulations so propagated, after any interval of time, constitutes the wave in its new position. Hence, if there be any part of the sea into which undulations are propagated more slowly than they are into the other parts, the wave in that part will not advance so fast as in the rest, and the line of the wave will there hang back. Thus, if the wave travel more slowly near the shores than in the wide sea, the wave-lines will bend backwards in those parts so as to assume a convex form; and the cotidal lines might resemble the curves I, II, III, IV, V, VI, VII, in the adjoining fig. 1.

Fig. 1.

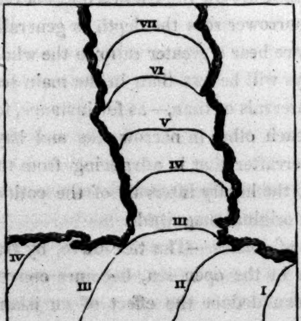
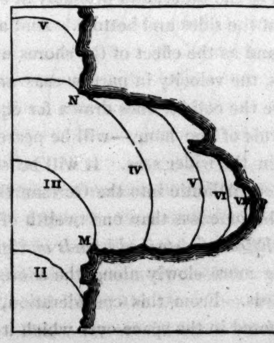


Fig. 2.



3. *Effect of arms of the sea and bays.*—In the same manner in which the undulation produced in our hypothetical southern ocean sent off a ramification

northwards into the inland sea, the undulation produced in this sea would send off a ramification into any lateral channel or inlet which might branch off from the main expanse. It will however be proper to consider this case a little further. Let there be a deep bay on the eastern shore of the inland sea, as fig. 2; when the undulation, travelling northwards, reaches the southern cape of this bay, it will be propagated eastwards into the bay as well as northwards, proceeding in all directions from the southern cape (M) till it meets the cape which forms the northern point of the bay (N); after which, the undulation in the main sea and in the bay will be detached from each other, and will each advance independently. And each of these undulations will again be affected by the form of the shores and other circumstances, in the same way as the main undulation. It is clear that if we proceed from the point N along the coast in either direction, we shall arrive at points where the tide is later than it is at N; the tide-wave separates into two at that point, and N *is a point of divergence of cotidal lines.*

Also, if P be the extreme point which the tide-wave reaches, the tide at P will be later than it is at the coast on either side of P. The tide-wave travels along the shore to P from each side, and P *is a point of convergence of cotidal lines.*

Also, the velocity with which the undulations advance depends upon the depth of the water, and probably in some measure on the friction and unevenness of the sides and bottom. And as in narrower seas the depth is generally less, and as the effect of the shores will there bear a greater ratio to the whole forces, the velocity in narrow seas and bays will be less than in the main sea. Hence the cotidal lines drawn for equal intervals of time,—as for instance, for intervals of one hour,—will be nearer to each other in narrow seas and bays than in the wider seas. It will be seen hereafter that in advancing from the southern Atlantic into the German Ocean, the horary intervals of the cotidal lines become less than one twelfth of their original magnitude.

4. *Effect of detached islands and groups of islands.*—The tide-wave, by travelling more slowly along the shore than in the open sea, becomes convex forwards. From this consideration, we can deduce the effect of an island interposed in the space over which it is to pass. In fig. 3. the I and II o'clock lines are not at all affected, or very slightly, by the island; the III o'clock line is held back so that it meets the shores of the island, although in other

parts of the ocean it is in advance of that place. The same is the case with the IV o'clock line, but the advance being greater, the two convex portions at the two ends of the island are turned towards each other; in the V o'clock hour line these portions touch; and thus the line may be considered as formed of two, which meet at the point of contact just mentioned, one line having its two ends on the shores of the island; the other line running across the ocean like the uninterrupted lines, but with an indentation towards the island. After this time these two lines give rise to two separate waves, 6 and VI; the former moving in a retrograde direction towards the island; the latter moving forwards, and gradually obliterating the indentation produced by the island.

It appears in this way that there is *a point of divergence of cotidal lines* on the side of the island which is towards the coming tide-wave, and *a point of convergence* on the opposite side.

Fig. 3.

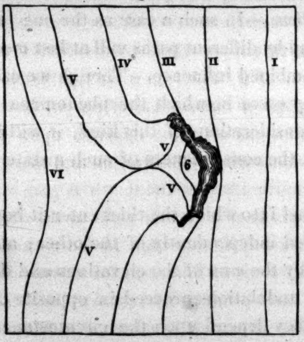
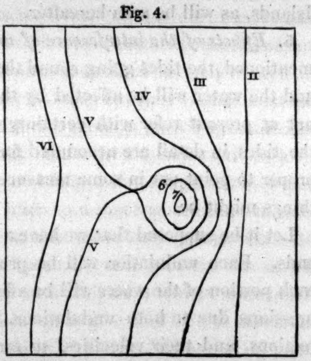


Fig. 4.



And if there be shallower parts of the ocean, not connected with any land, or connected only with small islands, the effect upon the form of the cotidal lines will be of the same nature, but may go still further. See fig. 4. In advancing upon such a part of the ocean, the cotidal lines immediately behind it will be brought closer together, while to the right and left of the place they will proceed without a corresponding thronging. Hence the cotidal curve on the two sides will advance beyond the islands, while it cannot pass directly over the islands themselves. The undulation will be propagated from the right and left into the space beyond the islands, and the convexities of the cotidal curves will at last meet there, as is shown in the cotidal curves V V in fig. 4. By this means the islands are surrounded by a ring-formed wave, which will advance towards the centre of the ring, and thus produce concentric ring-formed cotidal lines, as 6, 7. In the mean time, as the wave advances beyond the islands after its two parts are united, the indentation in its convexity will be gradually obliterated; and after a time, if the sea be of sufficient extent and of nearly uniform depth, the curve will again become continuously convex.

If the passage on one side of an island be much wider and deeper than on the other, the tide-wave, travelling much more rapidly in the wider space, and always extending itself laterally where there is room, may go round the island on its open side, and return on the other side in a direction opposite to its original one. This is the case with the tide-wave which visits the British Islands, as will be seen hereafter.

5. *Effects of the interference of undulations.*—In such a case as the one just mentioned, the tides going round the island by different paths will at last meet, and the water will be affected by their combined influence. Though we cannot at present refer with certainty to any cases in which the phenomena of the tides in detail are accounted for by considerations of this kind, it will be proper to point out in some measure what the consequences of such a state of things might be.

Let it be supposed that we have a channel into which the tides enter at both ends. Each undulation will be propagated independently of the other; and each portion of the water will be affected by the sum of the elevations and depressions due to both undulations. The undulations proceed in opposite directions, and their velocities, so far as they depend upon the circumstances of the channel, will be equal. If we also suppose their total elevations to be equal, we may trace the result in the following manner:

I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	I	II	III
XI	X	IX	VIII	VII	VI	V	IV	III	II	I	XII	XI	X	IX
XII	XII	A	VI	VI	VI	VI	VI	B	XII	XII	XII	XII	XII	C

Let the numerals in the upper line represent the positions of the wave which advances from left to right at those hours; the figures in the next line the positions of the other wave; and the third line the hours of the resulting tide. Two tides arrive at the same point, one having its greatest elevation at XI, and the other at I o'clock, the corresponding low water being at V and VII respectively. But it is clear that from XI to XII, the tide of which the hour is I, will rise faster than the tide of which the hour is XI falls, because the latter is close to its maximum; and thus, by the combined effect of the two undulations, the water will continue to rise; at XII the joint tide will be highest; and in the same way it will appear that the lowest water at this place will occur at VI o'clock. In like manner the two tides which would take place separately at X and at II will produce a joint tide at XII, the intermediate hour, and low water at VI; but the high water will be less high than the former, because both the component tides are further from their maxima. The two tides which would take place at IX and at III might produce a joint tide at XII also, if there were no recurrence of the tides; but the tide which comes at IX returns again twelve (tidal) hours afterwards: the fall of one of the component tides exactly counterbalances the rise of the other; and at this point there is no tide at all, the water remaining always at the same level. Beyond this point, we come to a place where the times of the compound tides are IV and VIII, which would produce high water at the intermediate hour VI; then to a point where the times are V and VII, and the high water (which will be greater than the last) still at VI; then to a place where the two tides coincide at VI, and the tide will be still at that hour, and greater than the adjacent tides; the times VII and V, VIII and IV still give the tide at VI, but successively less and less; the times IX and III give no tide, for the same reason as before. After this point, X and II again give XII for the tide hour, which continues while the component tides are XI and I, XII and XII, I and IX, II and X; till III and IX again obliterate the tide; and after this, it again happens at VI. Thus from A to B the compound tide is at VI, from B to C it is at XII, and so on alternately; and in proceeding from A to B or from B to C the tide increases, attains a maximum amount at the middle point between the extreme points, and then diminishes. In the case of such an interference of two tides, it thus appears that there are not progressive cotidal lines, but stationary cotidal spaces. The portions of water from A to B, and from B to C, rise and fall alternately, while the points A, B, C neither rise nor fall. There are two tide hours only (in this instance VI and XII) over the whole extent of the channel, while some points have no tide at all.

The circumstances just described result from the interference of two tides which move in opposite directions, the two being equal in amount at every point which they reach. If two tides interfere without being equal, they will still produce a compound tide, of which the circumstances may be altogether different from those of a simple tide.

It must be observed, that the propagation of undulations in any direction does not imply necessarily a movement of the water in that direction; therefore the concourse of two undulations coming in opposite directions does not imply the meeting of two currents. In the case where the cotidal lines approach from opposite quarters and come together, we may, if we choose, speak of "the meeting of the tides"; but it is to be recollected, that such a phrase is then used in a different sense from that in which it is often applied; for in common language, the *tides* are said to *meet* at those places where the *current* which brings the flood changes its direction, coming on one side from one quarter, and on the other side from the opposite quarter. How far this *meeting of the tide-currents* may be expected to *coincide* with the *meeting of the cotidal lines*, is a question to be considered hereafter.

We shall endeavour to trace the course of the cotidal lines according to which the tide is actually propagated in the Ocean; and it will be seen that they exemplify most of the preceding remarks. But before we proceed to do this, it will be proper to make a few remarks respecting the tide observations which we possess, and which must be our materials for such an attempt.

Sect. II. *On the causes of inaccuracy in Tide Observations.*

1. *Difference between the time of High Water and the time of Slack Water.*—When we begin to compare the times of the tide at different places as stated by the best authorities, in the usual manner, we find a great number of cases which seem to interfere altogether with any notion of obvious connexion and simple laws prevailing in this class of facts. Thus, if we refer to these authorities as recorded on Mr. Lubbock's chart, published in the Philosophical Transactions for 1831, we find many instances where places very near each other are marked with very different hours. The hour at the Eddystone is

marked VIII, while on the adjacent coast of Devonshire it is V; on the coast at the Land's End it is IV½, but at a very little distance it is V½. On the north coast of Ireland IV½ and IX½ stand close to each other; and similar discrepancies appear in many other places. Such discrepancies seem at first to make it impossible that the cotidal waves should have any regularity in their form and order. But there is an additional difficulty with regard to these cases: we can hardly conceive how they are possible on the principle of the water finding its level. If the time of high water at Plymouth be 5^h and at the Eddystone 8^h, the water must be falling for three hours on the shore, while it is rising at the same time at ten or twelve miles distance; and this through a height of several feet. We can hardly imagine that any elevation in one of the situations should not be transferred to the other in a much shorter time than this.

There is, in fact, no doubt that most, or all the statements of such discrepancies, are founded in a mistake arising from the comparison of two different phenomena; namely, the *time of high water*, and the *time of the change from the flow to the ebb current*. In some cases the one, and in some the other of these times has been observed as the *time of the tide*, and in this manner have arisen such anomalies as have been mentioned.

The time of the change of current, or the *time of slack water*, as it may be termed, *never* coincides with the *time of high water*, except close in upon the shore, and within its influence; the interval of the two times is generally considerable. Great confusion has been produced by these two times not being properly distinguished; so great indeed, that it has made almost all the tide observations which we possess of doubtful value for our present purpose.

The persuasion that, in waters affected by tides, the water rises while it runs one way, and falls while it runs the opposite way, though wholly erroneous, is very general. Thus it is often stated that the time of tide in the British Channel must be three hours later in the mid-sea than it is on the shore, because the easterly current continues running three hours after the time of high water on the coast. It would be easy to multiply instances of the perplexities into which various persons have been led by this assumption. It may suffice to quote one mentioned by Mr. Stevenson in his account of the Bell Rock Lighthouse. The waters of the river Dee at Aberdeen, even at the entrance of the harbour, have almost a constant current seaward, notwithstanding the opposite direction of the flood tide of the ocean. One of Mr. Stevenson's assistants, a very intelligent shipmaster, stationed at low water mark, "continued at his post while the water flowed up to his middle; and when accosted about his situation, he significantly observed, that it was rather extraordinary, as the stream had never ceased to indicate the continuance of the ebb-tide while the water was still rising upon his body."

In this case the current by which the water was affected was not the effect of the tide alone, which generally produces two opposite currents, alternating, and nearly equal in their duration. But in the case of such alternate tide currents also, the time of change of their direction is not in general the time of high water. This indeed is well known to intelligent seamen, who have accordingly phrases by which they express the relation of these times. When the stream in the offing which brought the tide continues to run for three hours after it is high water, it is said to make "tide and half tide *." But though seamen have thus noticed the fact, their recorded observations are very far from always recognising the distinction, as will be seen when we proceed to examine them. In the cases above mentioned, 5 o'clock at Plymouth is the time of high water there; and 8 o'clock at the Eddystone is the time of the slack water or change of current at that rock; the time of high water there being, on the average, a few minutes earlier than it is at Plymouth. In like manner $4\frac{1}{2}$ is the time of high water at the Land's End, and $6\frac{1}{2}$ is the time of a certain change in the direction of the current which is assumed to be the change from flow to ebb: in this latter case, however, the assumption is somewhat arbitrary, as the motion of the current is not alternately opposite, the change of its direction being rotatory.

A very little consideration is sufficient to show that in a bay or harbour the time of high water must coincide with the time of slack water, and that in the open sea these times will not coincide. In harbour, the water which flows in at the mouth has no egress by any other part; it therefore accumulates and rises as long as the ingress continues. But in a channel open at both ends, the case is quite different. The rise or fall of the water in any part in such case depends upon this;—whether the water comes faster than it goes away;

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* Captain White adds that when the flood stream runs an hour and a half after high water, it is termed "tide and quarter tide;" and when it runs three quarters of an hour after high water, "tide and half-quarter tide."—Survey, p. 269.

or the reverse. Let the flood in a certain channel come from the west; then at any given point the surface will rise by the water which the flood current brings, and sink by the water which the same current takes away: it will rise therefore if the current, at points to the westward of the given point, be more rapid than it is at points to the eastward of the given point. And the surface will be highest at the given point, when the current is equally rapid at the easterly and at the westerly points; and obviously not when there is no current. And the same is true of the times of lowest water at the given point, considered with reference to the current in the opposite direction. If the channel were of equal width, and the tide of equal height through its whole length, the time of high water would coincide with the time of greatest velocity of the current; and the change of the current would take place at the mean time between high and low water, and consequently six hours after high water. In like manner the time of low water would occur when the current was most rapid in the opposite direction. If the channel be narrower to the eastward, the time of slack water will be later than the time of high water by a smaller interval; and this diminution of the interval goes on till the times coincide, when there is no outlet, as has already been said.

In open seas the time of the change of the direction of the current is of more consequence to the seaman than the time of high water; and accordingly the first has been often recorded, when the other has not: hence the observations, by means of which we are to trace the cotidal lines are more scanty than they at first appear; and, what is worse, are often very doubtful in their meaning. But having pointed out the nature and frequency of this ambiguity, the anomalies which may occur in tide observations will be less perplexing than they were while such a source of confusion was not adverted to; and we may sometimes in such cases be able to distinguish the true from the erroneous statement. The time of high water is the fact which is most important for our present purpose; but that and the time of slack water ought to be both separately noted in all careful observations of the tides.

2. *The change of the Moon's angular distance from the Sun in the course of the day.*—The times spoken of in the preceding paragraphs as recorded for different places are the *hours of the tide on the days of full and new moon,* which times are often called the *establishments* of the places to which they belong.

This establishment is supposed to regulate the time of the tide on all other days of the lunation, the time of the tide being primarily governed by the moon. This supposition, which is true as a first approximation, assumes that the tide always occurs at the same hour-angle from the moon. But the hour of the tide on any day expresses its hour-angle from the sun; and as the moon changes her right ascension by about 48 minutes every day, the observed hour of the tide being given, on the day of full and new moon, the hour-angle from the moon may be different according to the time of the day when the conjunction takes place, compared with the time of day when the observed tide takes place. Thus if the conjunction take place at 1 o'clock in the morning, and the observed tide at 11^h at night, the distance of the tide from the sun is eleven hours; but at 11^h at night the moon is to the east of the sun by her motion in 22 hours, which is 44 minutes of hour-angle, and therefore the tide is only 10 hours 16 minutes behind the moon. But if the observed tide take place at 1^h in the morning and the conjunction at 11^h at night, the moon, at the time of the tide, is 44 minutes to the west of the sun, and the tide is 1 hour 44 minutes. In the former case the establishment is 44 minutes less, in the latter 44 minutes more, than the observation of the *hour* of the tide gives it.

If the time of tide were observed to be 6^h in the evening, the conjunction being at 1^h in the morning, the true establishment is 5^h 26^m, but if the tide be at 6^h in the morning and the conjunction at 11^h in the evening, the true establishment is 6^h 34^m. In this way it appears that an observation of the hour of the tide on the day of new or full moon leaves an uncertainty of 1 hour 8 minutes (and it may be more) as to the establishment, if we do not take into account whether the morning or afternoon tide was observed, and at what hour the conjunction or opposition of the moon took place.

In addition to this ground of uncertainty, the time of high water may often be doubtful to the extent of ten minutes or a quarter of an hour, from the want of precision in the observation; and as this error may occur in opposite directions at two different observations, and may be combined with the variation just mentioned, we may have thus two establishments different by above an hour and a half, collected from observations of the same place.

We cannot obtain any considerable accuracy in the determination of the establishment, without using numerous observations; and in this case the mean of the morning and evening tide-hours may be taken, the effect of the intervals by which the conjunctions and oppositions of the moon precede and succeed noon being supposed to counterbalance each other. In this case also the errors of observation may be supposed to be corrected in the average. But if we have to collect the establishment from a few observations, it will be proper to calculate in each case the hour angle by which the tide is distant from the moon.

3. *The semimenstrual inequality of the establishment.*—It has been already said, that the supposition that the tide depends on the moon alone, is a first approximation only. The time of high water does not follow the moon's transit by the same interval at every period of the lunation; the interval is sometimes greater and sometimes less than that *corresponding* to the new and full moon, and is regulated by the distance of the moon from the sun. The following is the mean state of this variation. When the moon and sun are in conjunction, the corresponding tide follows the moon by its mean interval. When the moon is at various hour angles after the sun, the following are the mean corrections of the mean interval, negative and positive[*].

Hour angle of the moon,	0	1	2	3	4	5	6	7	8	9	10	11	12 hours.
Correction of the establishment,	0	−16	−31	−41	−44	−31	0	31	44	41	31	16	0 minutes.

Thus, if the establishment corresponding to the new and full moon be 6 hours, the time of the corresponding high water when the moon is 1 hour from the sun, will be $5^h\ 44^m$ after the moon's transit; when the moon is 2 hours from the sun, the time of tide will be $5^h\ 29^m$ after the transit; and so on. When the moon is 6 hours from the sun, the corresponding time of high water will again coincide with the mean, after which the interval of the transit and tide will be greater than the mean, till the next conjunction or opposition; and then the same cycle recurs.

Hence, if the establishment were collected from any observation of the tide not corresponding to the day of new or full moon, it would be liable to an error. If the establishment were 6 hours, by an observation made when the moon's hour angle was 4 hours, and compared with the time of the moon's transit, it would appear to be $5^h\ 16^m$; but by an observation made when the moon's hour angle was 8 hours, it would appear to be $6^h\ 44^m$.

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[*] The law and magnitude of these numbers depend on the relative effect of the sun and moon upon the tides; the amount varies with the declination of the sun and moon, and with the moon's parallax.

This cause of difference in the results would be avoided by making the observation of the tide corresponding to new or full moon, or by applying the proper correction, according to the preceding Table, when any other tide was observed. The chance of error would however be removed more effectually by taking the mean of all the intervals between tide and transit, corresponding to half a lunation, or to any whole number of half-lunations.

4. *The correction of the establishment for the age of the tide.*—In the preceding paragraph we have spoken of the tide *corresponding* to new or full moon, and not of the tide which takes place *on the day of* new or full moon. The latter however is that which has been commonly observed for the purpose of determining the establishment of any place; but it does not coincide with the former, and there are certain anomalies in the tide records, depending on this difference.

The tide which comes to the shores of narrow and long seas is not immediately produced by the moon, but is derived from the tide in the main ocean; its circumstances are governed by those of the primary tide from which it is derived, and whatever interval may be employed in its transfer, it is regulated by the position which the sun and moon had at the time when they determined the primary tide. Now, this time may have been one, or two, or more days, before the tide reaches the place where it is observed. Thus, the tide on the shores of North America and Spain is determined by the configuration of the sun and moon at a day and a half previous; the tide in the port of London appears to be two days and a half old when it arrives. This circumstance affects the determination of the establishment from observations, in a manner which must be explained.

Since the tide at London is determined by the position of the sun and moon $2\frac{1}{2}$ days before it occurs, the moon must then have been more to the west of the sun by an hour angle of 2 hours (her motion in Æ. in $2\frac{1}{2}$ days,) than she is when the tide arrives. Hence, the tide which happens on the day of full moon corresponds to the period when the moon was in Æ. 2 hours west of the point opposite to the sun, or 10 hours east of the sun. Therefore, by the Table in last page, the tide is 31 minutes later than the mean interval of tide and moon's transit. The tide is observed to take place at 2 o'clock on the days of new and full moon, therefore $1^h\ 29^m$ is the *corrected establishment* for London.

In general, however, the establishment is defined to mean the hour of high water *at* new and full moon. We shall call this the *vulgar establishment.* Observations of tides have generally been directed to the object of determining this vulgar establishment, which, it appears, by what has been said, is not a corresponding quantity at different places. The mean of all the intervals of tide and transit for a half-lunation is the corrected establishment; the vulgar establishment is greater than this by a quantity depending on what may be called *the age of the tide*, namely, the length of time which has elapsed since its real or theoretical origin.

The corrected establishment may be determined, as has already been said, by taking the mean of the intervals of tide and moon's transit for any whole number of half-lunations. But we may observe, that it might be collected immediately from the vulgar establishment, if we had obtained a first approximation to the distribution of cotidal lines upon the surface of the ocean; for the age of the original tide in any part of the open ocean being known, the age of the tide derived from the original tide in any other part would be known from the number of intervening cotidal lines. Thus, if the tide on the coast of Spain be a day and a half old, the tide on the coast of Norfolk must be nearly two days and a half old, inasmuch as there are nearly 24 horary cotidal lines in the interval, if we follow the sea round the north point of Scotland, which is the course by which the tide reaches the eastern coast of England.

Since the recorded tide observations are liable to the very great inaccuracies and even ambiguities which have been pointed out, it may easily be conceived that we cannot at present deduce from them the course of the cotidal lines with accuracy and certainty. We may add to this, that our observations are very scanty in extent, compared with those which such a use of them would require; there are many seas and coasts where we have no information at all respecting the times of tide. As, however, an attempt to use such observations as we have, may, perhaps, lead to the collection of more numerous and more accurate ones, I shall endeavour to draw a first approximation to the course of the cotidal lines, begging the reader to bear in mind that from the nature of our materials it must be imperfect, and may be widely erroneous.

Sect. III. *Discussion of the Tide Observations now extant.*

There are various sources and collections of information on the subject of tide observations. The most important essay towards a complete collection is that contained in the 4th volume of LALANDE's Astronomy. In this the author has not adverted to the causes of confusion which have been pointed out; and his survey of the existing information at that period led him to terminate his statement with an earnest request that all persons having the opportunity would endeavour to render our knowledge more complete. In various books of astronomy and navigation, there are lists of the establishments of places in different parts of the world; and in Sailing Directions for considerable tracts of the ocean, statements of the same kind are collected. Among works of this nature I may mention NORIE's Epitome of Navigation, and PURDY's three Memoirs, that on the Atlantic Ocean, that on the Ethiopic or South Atlantic, and his Columbian Navigator. The Sailing Directions for more limited spaces also contain such statements. The nautical surveys of various navigators supply the establishments of places recorded in the charts, or in the accompanying remarks; I may notice particularly the Surveys of the Australian coasts by Captains FLINDERS and KING, and of Patagonia by the latter officer, in addition to many others nearer home. The "Remark Books" of various ships contain many such observations. These latter documents, and a large proportion of the information contained in the various surveys which have been almost unintermittingly carried on by naval officers of this country for a long course of years, exist in manuscript in the Admiralty.

With regard to these materials—I have, by the kindness of the Hydrographer, Capt. BEAUFORT, been allowed the free use of the charts and manuscripts belonging to his department, without which advantage, indeed, I should hardly have been able to make the present attempt, imperfect as it may be.

I shall begin by considering the tides of the Atlantic, which are, at least in their main features, of a derivative kind, and are propagated from south to north according to the laws of undulations in a limited sea, as explained in Sect. I. of this memoir.

The cotidal lines, which I shall draw by means of the data I am now about to discuss, are drawn through or near the points for which the time of high water is supposed to be ascertained; and are moreover drawn so as to possess as much regularity and similarity in their form and intervals, as the data will allow. By this means the lines pass across parts of the ocean where no tide observations have been, or perhaps can ever be made. The tide hours in such parts are to be considered as obtained by the interpolation to which the forms of the cotidal curves conduct us.

The East Coast of the Atlantic.

If we look at Mr. Lubbock's chart of the world, or at any other good general view of the tides along the west coast of Africa, Spain, Ireland and Scotland, it will appear tolerably certain, from the tide-hours given for different places, that the tide-hour which is about $1\frac{1}{2}^h$, Greenwich time, at the Cape, becomes successively, in going northwards, 2^h, 3^h, 4^h, 5^h, 6^h, 7^h, 8^h, 9^h, 10^h, 11^h, 12^h which is the hour about Cape Blanco, and goes on to 2^h, 3^h, 4^h, 5^h, &c. on the western coasts of Europe.

For the greater part of this coast we have not the means of determining the hour with much additional accuracy. Concerning the Cape of Good Hope, where we might have expected the establishment to be well and accurately known, I have not been able to obtain good information. Certain "Observations" sent from that place to the Admiralty are obviously not to be depended on. The "Tide Table calculated for Table Bay" inserted in the South African Almanac for 1832, must be very erroneous for a large proportion of days, even if the mean establishment be right. This will be clear when it is stated that the Table is constructed by assuming certain tide-hours for each day of a cycle of 30 days, and by applying this cycle, with no exact regard to the day of the moon's age, and with no regard at all to the hour of her transit. In Norie's Epitome of Navigation, the establishment of Table Bay is given as $2^h\ 25^m$, and subtracting $1^h\ 14^m$ of east longitude, we have $1^h\ 11^m$ for the establishment reduced to Greenwich time. I shall for the present adopt this value.

The statements concerning the tides at St. Helena are various. Norie's Epitome gives the time for James Town, as $1^h\ 30^m$. Dr. Maskelyne in 1761 made a series of observations on the tides during the months of November and December, (Phil. Trans. 1762, p. 586.). These are very irregular, which is the less surprising as the observation was made by means of a post fixed in a part of the harbour where the waves were a foot or two high. I have compared these observations with the times of the moon's southing in the *Connaissance des Tems* for 1761, and find that they give a mean establishment of about 2^{h}.

Certain observations made at this island in September 1826 by Lieut. Johnson have been communicated to me; they are very irregular, probably in consequence partly of uncertainty arising from the smallness of the tide, the greatest rise not being much more than three feet. The establishment resulting from these appears to be about 2^{h}.

There are, however, among the Admiralty papers some observations made by General Walker at the request of Mr. Fallows, which from the precautions with which they are described to have been made, and from the accordance of the results, appear to be more worthy of confidence than any of those previously mentioned. These have been examined by Mr. Dessiou, and have given as the mean of the semilunations, fourteen in number, 2^{h} 55^{m} for the hour by which the tide follows the transit of the moon. I shall therefore adopt this as the establishment at St. Helena. The difference of longitude from Greenwich is so small that no correction on that account is requisite.

The tide hour at Ascension Island is stated in two "Remark Books" of different ships, as 4^{h} and 5^{h} 30^{m}. I have been furnished with a record of observations made by Capt. R. Campbell, R.N., at various periods from March 1820 to August 1821, by which it appears that the establishment is about 5^{h} 5^{m}; adding to this 57^{m} of west longitude, we find that the cotidal line of 6^{h} 2^{m} touches this island.

Returning to the coast of Africa, and taking for our authority Mr. Lubbock's chart, which in this part he states to be founded on Captain Owen's surveys, and referring also to the list in Norie's Epitome, we find that we have the following order of latitudes and establishments.

	Latitude.	Establishment.
Saldanha Bay	33° 2′ S.	2^{h} 0^{m}
St. Helena Bay	32 42 S.	2 30
Cape Serra	22 0 S.	3 0
St. Paul de Loando	8 48 S.	4 30
Gaboon River	0 30 N.	5 0
New Calebar River	4 22 N.	5 0

The coast during this space runs nearly north and south, so that the correction for longitude will not much affect the differences of these times; and hence it appears tolerably certain that the tide-wave travels from the Cape of Good Hope to the bottom of the Gulf of Guinea in something less than four hours.

We have the following statements for points in the Gold Coast.

	Longitude.	Establishment.	
Cape Coast Castle	4′ E.	3^h 30^m	Norie.
Cape Three Points	9 W.	3 30	Lubbock.

This would seem to imply that the 3^h 30^m cotidal line crosses from the neighbourhood of St. Paul de Loando to the Gold Coast, which would not be impossible, but appears to be inconsistent with the tide hours at St. Helena and Ascension: I shall therefore not draw it so till we have more certain information.

The following tide hours are given for places in Fernando Po by Norie.

George's Bay	4^h 0^m
Goat Island	4 0
Cape Buller	4 15
Also Island of St. Thomas .	3 25
But	5 30 in Mr. Lubbock's Chart.

The last-mentioned observation falls in most easily with the general form of the lines. I shall for the present suppose it to be correct, and the others inaccurate.

In proceeding westward along the coast I do not know any statements of the tide hour till we come to Sherbro Island, the Islands of Bananas, Cape Sierra Leone, and the Islands of Los. These places lie between latitude 7° 35′ and 9° 30′ N., and have west longitude about 52^m of time. Their establishments are stated as follows:

Sherbro Island .	5^h 53^m; and reduced to Greenwich time, 6^h 45^m Lubbock.
Bananas Islands .	8 15 Purdy *.
Sherbro River . .	8 0 Norie.

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* Memoir to accompany a new Chart of the Atlantic Ocean, 1820.

	Establishment.		Greenwich Time.
Cape Sierra Leone . .	7^h 30^m	Norie . . .	8^h 22^m, but 7^h 45^m Lubbock.
River Sierra Leone .	8 15	Purdy.	
Isles of Los	9 0	—— . . .	9 56, but 7 30 Lubbock.
	7 40	Boteler*. .	8 32

Taking Captain Boteler's times as more probable in this case, we perceive that the 7^h tide line must fall somewhere in the neighbourhood of Sierra Leone. Thus, if we had adopted the hour $3\frac{1}{2}^h$ stated for Cape Coast Castle, it would follow that the tide-wave occupies $3\frac{1}{2}$ hours in moving from that place to Cape Sierra Leone,—an interval of less than one hour of longitude. On the other side of the Gulf of Guinea the tide-wave in the same time, namely, from 1^h to $4\frac{1}{2}^h$, had moved through about forty degrees of latitude.

As we proceed to the north the times become later, though without any apparent regularity. We have the following hours given:

	Latitude.	Establishment.		
Bathurst	13° 28′ N.	8^h 10^m	Norie.	Boteler.
River Gambia (within)	13 39	11 45	——	
Goree	14 40	7 48	——	
		7 0	Adanson (Savans Etrangers, ii.605.)	
Cape Verd	14 43	7 45	Norie.	
Senegal (bar) . . .	16 1	10 30	——	
Cape Blanco	20 50	9 45	——	
River Ouro	23 51	12 0	——	
Cape Bojador . . .	26 7	12 0	——	

The observations made within the rivers Senegal and Gambia are apparently affected by the retardation due to the inlet of the river, and may be rejected; that at Bathurst is probably affected in the same way; perhaps that of river Ouro. The longitude of all these places is about 1^h west. Hence I shall take for the establishment of

Cape Verd	8^h 45^m	Greenwich time.
Cape Blanco . . .	10 45	
Cape Bojador . . .	1 0	

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* Sailing Directions for the West Coast of Africa.

If these data can be depended upon, the 12^h tidal line meets the coast of Africa somewhere very near latitude $23\frac{1}{2}°$.

The anomalies of the recorded tide hours of the coast of Africa to the north of this are so perplexing, that I shall in the first place proceed to the coast of Spain, where the order is clearer.

The tide at Cape St. Vincent is stated to occur at $2^h\ 15^m$; at Cape Ortegal, at 3^h; and at nearly the same time on the south coast of the Bay of Biscay. Opposite Brest the establishment is $3^h\ 30^m$, which is also stated to be the tide hour of Valentia at the S.W. corner of Ireland. Beyond this line of $3^h\ 30^m$ we are able to trace the course of the tidal wave in great detail on the shores of this and the neighbouring countries; but we shall first endeavour to complete our view of the Atlantic.

West Coast of the Atlantic.—I shall begin the examination of the tides on the western shores of the Atlantic Ocean, from Cape Frio, in lat. 22° 59′ S. We possess observations of the tide at this point, which are probably pretty exact, having been made during a stay of considerable length, by the persons engaged in the operations carried on for the purpose of recovering the lading of the treasure-ship Thetis, which was sunk there. In a chart of the port of Cape Frio, by Lieut. H. KELLET, which appeared in the Nautical Magazine for April 1832, the establishment is stated to be $1^h\ 40^m$; and as the longitude is $2^h\ 48^m$ W., the tide hour, Greenwich time, is $4^h\ 28^m$.

M. ROUSSIN, who surveyed this coast in 1819–1820, also gives the hour at the Bay d'Espirito Santo, in lat. 20° 18′ S. as 3^h; and at the Island of St. Sebastian, to the south of Cape Frio, in lat. 23° 50′, as 2^h; but at Santa Catharina and Rio Janeiro as $2^h\ 45^m$ (lat. 27° 30′); so that from St. Sebastian the hour appears to be later both to the north and south, and there is a point of divergence in that neighbourhood: on all accounts Cape Frio is its most probable position.

PURDY (Ethiopic Memoir, p. 59,) gives the establishment at Cape Frio as 9^h; and the person who examined that shore after the loss of the Thetis (Admiralty MSS.) states the time of tide as 9^h; but it is clear from the context, that he means the time of change in the direction of the stream; which naturally attracted the greatest share of his attention, the shipwreck having been occasioned by the effect of currents.

Proceeding from Cape Frio northwards along the coast of Brazil, I find the following statements:

	Lat.	H. W.	H. W., Gr. T.
	° ′	h m	h m
Bahia or St. Salvador....	13 0 S.	4 15 NORIE, ROUSSIN.*..	6 49
			6 0 LUBBOCK.
Pernambuco............	8 4	7 15 NORIE............	9 35
			7 0 LUBBOCK.
Paraiba	....	4 15................	6 35 ROUSSIN.
Cape St. Roque	5 28		
Fernando Noronha	3 56	4 0 NORIE.	6 15 LUBBOCK.
Ciara, or Seara..........	3 45	4 40................	7 14 ROUSSIN.

The establishment given by M. ROUSSIN for Pernambuco appears much more probable on all accounts than NORIE'S, and I shall adopt it.

Beyond Cape St. Roque the coast trends away to the westward. The following times of tide are given:

	Long.	H. W.	H. W. Gr. T.
	h m	h m	h m
Cape St. Roque	2 21 W.		
Jaguarybe..............	2 31	6 0	8 31 Sailing Directions.
Maranham..............	2 56	7 0 NORIE, Brazil.	9 56 Also ROUSSIN.
Para..................	3 14	12 0 NORIE, Epit.	
(Mouth of river Amazon).		4 0 NORIE, Brazil.	
Cayenne	3 29	4 30 NORIE..........	7 59
		3 45 PURDY.	
Surinam. Bram's Point ..	3 41	5 30 PURDY..........	9 11
N. Amsterdam,..........	3 50	4 30	8 15 LUBBOCK.
(River Berbice)			
Demerary River	3 52	4 30 NORIE.	8 22
Barbadoes	3 59	scarcely perceptible (PURDY).	
Trinidad	....	4 30 PURDY..........	8 30 LUBBOCK.
——— Port Spain	4 6	6 30	10 36 NORIE.
St. Lucia	4 4	....	10 45 LUBBOCK.
Guadaloupe & Martinique.	4 7		
(Irregular)	....	6 45 PURDY..........	10 52

The tide at Para is perhaps retarded several hours by the inlet in which it stands, and that at Maranham probably by two or three hours. Supposing this to be the case, we see that the tide-wave advances to the westward and northward with tolerable regularity.

This agrees with RICHER'S observation (Acad. Par. vii. Part II. p. 320.), that the tides which are at $3^h 45^m$ at Cayenne, are earlier in proportion as we approach the equator.

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* Le Pilote du Brazil, 1819–1820.

Our materials are probably insufficient at present to enable us to make out with any degree of accuracy the course of the tide-wave among the Bahama Isles from Porto Rico to Florida.

I shall, however, insert the following statements, which I find in NORIE, annexing the rise of the water at spring tides, when it is given:

	H. W.		
Caribbees.	h	m	Ht. in Feet.
Saintes	6	45	
Porto Rico.			
St. Juan	8	20	1½
Hayti.			
Cape Haytien	6	0	2½
Puerto da Plata	7	30	3
Miraporvos	9	30	2
Lucayos.			
St. Salvador	3	50	
Providence Island	7	30	
Bury Island	7	30	
Bahamas.			
Exuma Bar	6	35	
Royal Island Harbour	7	45	3½
Pelican Harbour	7	30	4
Man of War Kay	8	10	4

I shall now endeavour to trace the progress of the tide-wave to and along the coast of North America.

The Bermuda Isles are placed in a position where an exact observation of the tides would throw light upon the course of the cotidal lines. Mr. LUBBOCK states the time of tide (Gr. T.) as 11^{h} 15^{m}, so that the 11 o'clock cotidal line must pass to the east of them. I have inspected, at the Admiralty, statements of observations made at the Naval Yard in Bermuda, in August and September 1832, from which it appears, that the time of high water at full and change is

7^h 18^m on the average, which added to 4^h 19^m, the longitude of the place, gives 11^h 37^m for the Greenwich time of tide. This discrepancy is slight.

For the coast of North America we have, besides other materials, the Tide Tables in the American Almanac. Taking the Almanac for 1831, and comparing the times there given with the moon's southing, we have,

	Lat.	Long.	H. W. after Moon's southing.	H. W., Gr. T.
	° ′	h m	h m	h m
Charleston....	32 45	5 20	7 22	0 42
New York....	40 40	4 56	9 0	1 56
Boston	42 20	4 44	11 38	4 22

Hence it appears, that along this coast the tide-wave runs steadily northwards; and this continues all the way to the mouth of the Bay of Fundy, where the tide from the north again makes its appearance, as is seen by considering the tides of Nova Scotia.

By observations of the tides made at Halifax Yard on the east side of Nova Scotia, and transmitted to the Admiralty, it appears that the establishment at that place is about 7^h 42^m. Proceeding southward along this coast, we come to the southern point of Nova Scotia, Cape Sable, where the tide hour is stated to be 8^h. (Purdy, Atl. Mem. p. 75.) Turning round this Cape we enter the Bay of Fundy, and just within it is Cape St. Mary, where the tide hour is 9^h: at the entrance of the Gulf of Anapolis, a little further up, the hour is 10^h; and in advancing further up to the head of the bay it becomes 11^h and 12^h.

On the opposite or western side of this inlet, at Penobscot and the neighbourhood, the establishment is 10^h 45^m; and as at Boston, further south, it is 11^h 30^m according to Purdy, or 11^h 38^m according to the Almanac, it appears that near Penobscot there must be *a point of divergence of cotidal lines*, the wave on the right hand running into the Bay of Fundy, and on the left into the Bay of Massachusetts formed by Cape Cod.

It will easily be conceived that the tide lines break into Chesapeak and Delaware Bays as separate inlets:—to follow these subdivisions of the lines would not suit either our present limits or materials.

The tides in the Bay of Fundy are very high, perhaps the highest in the world. In some places the spring tides rise sixty or seventy feet perpendicular. This is accounted for in some measure by the course of the cotidal lines. The tide which is at 7^h 42^m at Halifax, long. 4^h 14^m W., is on the cotidal line of 11^h 56^m; and this line again meets the coast of North America somewhere south of Charleston. The whole of the wave which advances from this line is made to converge by the shore of Nova Scotia on one side, and that of the United States on the other, into the entrance of Fundy Bay, and is thus accumulated to an unusual height.

It will be seen, when we come to speak of the tides of South America, that the tide lines on the coast of North America, which appear to be clearly established, agree with those on the coast of Brazil and Patagonia in several of those circumstances which in the last-mentioned case might appear most doubtful. In both instances the tail of the tide-wave, dragging along the shore, carries a large tide to the northward; and in the mean time the convex front of the wave, advancing more rapidly in the open sea, catches a more northern point of the coast, from which it sends another feebler tide southwards to meet the former.

We may add to the preceding data, that the 12^h cotidal line, which, as we have seen, passes near the coast of Nova Scotia, also passes near the coast of Newfoundland. At St. John's the establishment is 7^h 50^m (NORIE), which with the longitude 3^h 30^m, gives 11^h 20^m for the Greenwich time. Mr. LUBBOCK gives 11^h 30^m. In Placentia Bay, on the south side of the island, PURDY gives 9^h 15^m for the time, which agrees with Mr. LUBBOCK's 12^h 45^m Greenwich time.

I shall attempt further to pursue the tide-wave in its course up the gulf and river of St. Lawrence, in speaking of river tides.

We appear now, from what has been said, to have the means of drawing the cotidal lines of the Atlantic in a general way: but before we proceed to do this, there are some anomalies pertaining to the islands which require to be noticed.

Tides of the Atlantic Islands.

It is evident from what has been already said, that 12^h cotidal line runs obliquely across the Atlantic from the neighbourhood of Newfoundland, so as to meet the shore of Africa in about latitude 23° 30′ N. It appears also from the tides of the western shores of Europe, that the 2^h and 3^h cotidal lines advance in lines nearly parallel to this. But in the intermediate space lie the Azores, the

Madeira and the Canary Islands; and from what was said in Sect I., we should expect that the cotidal lines would be for a space thrown back and inflected in the neighbourhood of these islands, so that the tides will be later than they would be in that part of the ocean if it were deep water. It has also appeared that there may be detached spaces within which the tides are later than in the surrounding seas, occupied by *converging rings or loops of cotidal lines.*

It appears from the materials which we possess, that some of these circumstances do occur with regard to the islands of the Atlantic. The hours given for the Cape Verd Islands are, St. Jago 6^h, St. Nicholas 7^h, English Road, Bonavista, $7^h\ 30^m$; (NORIE'S Brazil, and PURDY.) If we consider the differences of these as too doubtful to be relied on, we may take 7^h as the establishment for this group, which gives $8^h\ 30^m$ for the Greenwich time; and it will be found that the $8^h\ 30^m$ cotidal line will pass without much flexure through this part of the ocean. The differences of time at different points of the group will modify these lines in detail.

The hours given for the Azores are, Fayal Road $11^h\ 30^m$, Terceira $11^h\ 45^m$, (NORIE); to which Mr. LUBBOCK adds, St. Michael $12^h\ 30^m$. Probably the former times belong more nearly to the general sea, and the latter (which is to the east of the others,) is perhaps affected by the retardation of the lines in a converging ring or loop. The time $11^h\ 30^m$ gives $1^h\ 30^m$ Greenwich time for the cotidal line. It will be seen by inspection that this line must here have been thrown behind its general course.

It is stated by NORIE and PURDY, that the hour throughout the Canary Islands is about 3^h. This shows a considerable retardation, and makes it necessary to suppose here, that the cotidal lines form a converging ring or loop. It is also stated by the same authorities, that the tide hour at Cape Geer on the coast of Africa, nearly opposite the Canaries, is $2^h\ 15^m$, and at Mogadore 4^h, which confirms the supposition of such a flexure.

At the same time, it would seem that this cotidal loop or ring is not very extensive; for at Madeira, which is only a few degrees north of Canary and a few degrees west of Cape Geer, the hours given are $12^h\ 4^m$ and $12^h\ 15^m$; and Cape Geer and Mogadore are so near each other, that it is hardly possible that the hours just mentioned for these places should be both right. It is probable, that somewhere between the Strait of Gibraltar and Cape Blanco there is a point of convergence, and that the 2^h, 3^h, and 4^h cotidal lines form loops among the Canaries, but we cannot at present pretend to determine their course more accurately.

The tide time at Cape Vincent ($2^h\,15^m$) places the cotidal line of $2^h\,51^m$ there, and the tides here must come principally from the west; the southern supply being much interrupted by the islands, as we have seen. And hence the tide must run into the gut of Gibraltar from the west, and we should expect to find the times later and later in approaching the Strait. The statements generally given are, Bay of Cadiz $1^h\,45^m$*, Portal of Cadiz $2^h\,15^m$, Zanta Island 12^h, Gibraltar $12^h\,15^m$. These hours appear to imply some unexplained anomaly or inaccuracy. It is scarcely possible that the cotidal line of $12^h\,30^m$, or even of 1^h, should have a flexure which carries it to Gibraltar. NORIE gives for Cadiz $2^h\,30^m$ (marking it *Obs.*), which is reconcilable with the general course of the lines, and for Cape Spartel, on the south of the Strait, he gives 3^h.

After approaching the coast of Spain, the cotidal lines appear to have a form in which the indentation produced by the islands is obliterated, as we should expect from the principles above laid down.

Tides of the North Sea.

We have already seen that the tide approaches the shores of our own and neighbouring countries from the south-west†; we have now to trace its course more particularly after it reaches the land.

The cotidal line of 4^h, Greenwich time, of which the direction is about N.W. and S.E., appears to be nearly that which first touches the coasts of Britanny and Ireland. We have,

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* LALANDE, p. 321, gives for Cadiz $1^h\,10^m$ "according to twenty-four observations which I received from Cadiz made in 1773 by M. TOFINO."

† An opinion formerly prevailed that the tides came to the coasts of England and Ireland from the north. Mr. MURDOCK MACKENZIE, in his Maritime Survey of Ireland, shows that the flood comes from the Atlantic to the west, divides itself into three streams at the S.W. points of Ireland and England, of which streams two meet again at the N.E. point of Ireland. SPENCE's Scilly Isles, p. 6, (1792).

On the coast of France,

	Time, H. W. h m	Long. W. m	Gr. Time. h m	
Ushant........	3 47	20	4 7	DAUSSY, (Conn. des Tems, 1834, p. 75).
Brest	3 48	18	4 6	——— Ibid.

And on the S.W. coast of Ireland,

	h m	m	h m	
Skellings......	3 30	40	4 10	SPENCE, p. 8.
Valentia	3 30	40	4 10	LUBBOCK.
Cape Clear	4 0	38	4 38	MACKENZIE.
			4 45	LUBBOCK.

In the last instance we already see the effect of the retardation produced in entering St. George's Channel.

We shall trace the tide along both this and the British Channel; but we may notice in the first place the Bay of Biscay. As materials for the lines on this coast, we possess the results of a "Reconnaissance Hydrographique des Côtes de France," undertaken by the corps of Ingénieurs-hydrographes, under the direction of M. BEAUTEMS BEAUPRÉ; of which the results, so far as the tides are concerned, have been stated by M. DAUSSY in a memoir inserted in the Connaissance des Tems for 1834. It might have been expected that the tide would take some time to reach the head of this bay at St. Jean de Leon. It appears, however, that the tide is nearly contemporaneous along the whole coast of the bay. The following times are given by M. DAUSSY, among many others:

	h m
Isle of Noirmoutier (Mouth of the Loire)	3 15
St. Martin-de-Ré (Isle of Ré)	3 40
La Rochelle	3 39
Tour de Cordovan (Mouth of the Gironde) .	3 59
Socoa (head of the Bay)	3 31

The $3^h\ 30^m$ cotidal line must therefore be nearly parallel to the coast of France.

Tides of the British Channel.

From this point there is no difficulty in tracing the course of the tide-wave along the British Channel, if in doing so we avoid the confusion already noticed, which is often made between the time of high water and the time of change from ebb to flow current; and if we also leave out of our account, or consider with proper allowances, tides in shores very much inbent, as the Bay of Poole and the Solent Sea. Thus, to determine the cotidal lines which pass across the Channel, we must not take the tides at Portsmouth, but those on the outside of the Isle of Wight.

Proceeding to the Scilly Isles, we find the time noted $4^{h}\ 10^{m}$, which gives Greenwich time $4^{h}\ 35^{m}$; Mr. Lubbock has $4^{h}\ 30^{m}$.

At the Land's End the time is $4^{h}\ 20^{m}$, which gives Greenwich time $4^{h}\ 43^{m}$. The marks VI$\frac{1}{4}$ and VI$\frac{1}{2}$ in Mr. Lubbock's chart refer to changes in the current.

Mr. Dessiou's Tide Tables for Plymouth, &c., give the following tide times for places near Plymouth, deduced from Plymouth, of which the mean establishment is $5^{h}\ 33^{m}$. (p. 4.)

	h	m
Mount's Bay and Lizard	4	30
Falmouth Harbour	5	15
Fowey Harbour	5	15
Cawsand Bay	5	23
Eddystone	5	15
Dartmouth and Torbay	6	0
Exmouth	6	25
Lyme Cob	6	0
Portland Bill	5	30
Weymouth	6	30

The last two are obtained from the same Tables, and deduced from Portsmouth, of which the establishment is $11^{h}\ 40^{m}$.

The Greenwich time at Portland Bill is hence $5^{h}\ 40^{m}$, and it is clear that the tides on each side of this point are bay tides. This is further confirmed by Mackenzie (Admiralty MSS.). He stationed observers at Portland Bill and at Weymouth, and found that high water at the Bill takes place above an hour sooner than at Weymouth.

As materials for the account of the tides on our own coast, we have many charts and surveys, executed by various persons, and especially by Murdoch Mackenzie and Græme Spence (1774 to 1792); and more recently a laborious survey by Captain Martin White, undertaken by direction of the Lords Commissioners of the Admiralty in 1812, of which the result has been printed, but is not yet published.

On the opposite side of the Channel we have,

	h	m	h	m
Morlaix			5	15
St. Malo			6	0
Mont St. Michel			6	30
Minquiers	6	0		
Jersey	6	10	6	0
Guernsey	6	30	6	0
Alderney	6	45		
Cherbourg			7	45

The first column is from Captain M. White's Survey of the Channel, p. 156; the second from the Annuaire du Bureau des Longitudes for 1833; the difference is not great. According to Captain White's times, the cotidal line advances in a northerly direction out of the bay of Mont St. Michel, for the Minquiers rocks are to the south of Jersey, as Jersey is of Guernsey.

The longitude of Guernsey is 10^m W. Hence it would appear, that the 6^h 30^m cotidal line crosses the Channel about that part, and probably runs towards the east, so as to pass beyond St. Malo on the French coast.

This, however, is not what we should expect from another circumstance, the great height of the tides at the head of this bay, (at St. Malo and Granville they are forty or fifty feet,) which would lead us to suppose that there is a point of convergence in this neighbourhood.

The progress of the wave appears to be retarded by the opposite projections of Portland Bill and Cape La Hogue, especially the latter. The 7^h line appears to pass near the latter of these promontories.

Proceeding eastward, we have the following times, which I take from Mr. Dessiou's Tide Tables for the English coast, and from the Annuaire for the French.

	h	m		h	m
Portland Bill	5	30	Le Havre	9	15
Needles Point	9	45	Dieppe	10	30
Bembridge Point	11	0			
Shoreham Harbour	11	15			
Beachy Head	10	15			

It appears that the 10^h 30^m line must pass nearly from Beachy Head to

Dieppe. The inbend between Beachy Head and the Isle of Wight appears to have *bay* tides, which run up to Portsmouth. The same is the case with respect to the inbend between the Isle of Wight and St. Alban's Head, which ends in Poole Harbour.

The following English tides are taken from Mr. Dessiou's Sailing Directions for the English Channel, p. 53 and 60; the French, from the Annuaire.

	h	m		h	m
Rye Harbour	10	36	Boulogne	10	40
Dungeness	10	30	Calais	11	45
Folkstone	10	45	Dunkirk	11	45
Dover	10	50	Ostend	12	20
South Foreland	11	0			
Deal	11	15			
Ramsgate	11	20			
North Foreland	11	15			
Margate Roads	11	40 Norie.			

The 11^h 15^m line passes from Deal to the nearest point of the opposite coast. The 12^h line, which falls between Dunkirk and Ostend on the continental side, passes within the inlet of the Thames.

The tides beyond this point are affected by those which enter the German Ocean from the north, and must be considered hereafter.

Tides of St. George's Channel.

I shall take these in the first place from Mr. Dessiou's Sailing Directions. We have, on the opposite and parallel coasts of Cornwall and Devon and of Ireland, the following times, (pp. 124, 137.)

	h	m		h	m
Land's End	4	30	Cape Clear	4	0
St. Ives Bay	4	30	Cork and Kinsale Harbours	4	30
Padstow	5	0	Between Waterford and Youghall	5	0
Lundy Isle	5	15	Hook Point (entrance of Waterford)	5	15
Barnstaple	5	30	Saltee Islands	5	50
Ilfracombe	5	30			

Also we find that the line now reaches the promontory of the Welsh coast which divides the Bristol and St. George's Channels: for we have (p. 129),

	h	m
Milford Haven	5	30
St. David's Head	6	0

I shall leave for the present the tides of the Bristol Channel, and follow the others.

The deep bay of Cardigan and Harlech, between St. David's Head and Bardsey Isle, will of course have *bay* tides. We have the following data (Dessiou, p. 137, 129.)

	h	m		h	m
Wexford Harbour	7	0	Fishguard Bay	6	30
Arklow	8	15	Cardigan Bay	7	15
Wicklow	9	0	New Keyhead	7	30
			Aberystwith	7	45
			Barmouth	7	45 Norie.

The lines are probably held back by the strait between the promontories of Carnsore and St. David's Head. In other respects they advance regularly to the north. The following Irish tides are from Captain Mudge's Sailing Directions for Dublin Bay, p. 16; the English, from Norie.

	h	m		h	m
Kingstown Harbour	10	17	Bardsey Island	8	15
Kish Bank Light	10	30	Caernarvon Bar	9	0
Balbriggen	10	40	Holyhead	10	0
Drogheda	10	40	Amlwch Point	10	30
Clogher Head	10	30	Orme's Head	10	30
Dundalk Bar	11	0	Liverpool	11	8
Cooley Point	10	40	Lancaster	11	15
Carlingford Bar	10	40	Whitehaven	11	15
Dundrum Harbour	10	30	St. Bees Head	11	0
Strangford Bar	10	30	Mull of Galloway	11	15
South Light	10	15	Mull of Cantire	10	30

It appears that there is a *point of convergence* somewhere near Dundalk, the cotidal lines north of this coming from the north. The general opinion, formed on the direction of the tide currents, that the "tides meet" at St. John's Point near Dundrum, agrees sufficiently well with this.

There is also a point of convergence on the English coast somewhere at the entrance of the Solway Frith: "the tides meet," according to the common opinion, at the Piel of Foudray, opposite the Isle of Man, where the tide has been known to rise thirty-six feet perpendicular, which is attributed to the accumulation of the two tides, (SPENCE, Scilly, p. 3.)

The Isle of Man shares the tides of the surrounding sea: we have in NORIE,

	h	m
Calf of Man	10	30
Douglas	10	30
Ramsey	10	30

It appears, therefore, that the 11^h cotidal line passes beyond this island on both sides.

Tides on the West of Ireland.

We must now trace the portion of the Atlantic tide, which, moving along the west coast of Ireland, comes round by the north and meets the tide of St. George's Channel. We have the following times at places along the west and north coasts, (NORIE).

	h	m
Skelling Rocks	3	30
Valentia Harbour	3	30
Dingle Bay	3	45
Tralee Bay	3	45
Loop Head	4	30
Galway Bay	4	15
Slyne	5	15
Achill Head	6	0
Donegal	6	30
Tory Island	6	0
Lough Swilly	6	30
Londonderry	6	0

These times succeed each other very regularly, but there may be some doubt whether they are the results of exact observation at each place. In the Nautical Magazine for November 1832, is a survey by Captain MUDGE of the Gola

Islands, which are about twenty miles south of Tory Island; and there the time is given $4^h\ 30^m$. Also in Captain HUDDART's Survey of this coast, the time between Tory Island and Mallin Head (which is to the N.E.) is given as 5^h. I think it more probable, therefore, that 5^h than that 6^h is the time for Tory Island.

East coast.	h	m
Fair Head	9	0
Carrickfergus	10	30
Belfast	10	30
Strangford Bar	10	30

It appears that in passing round Fair Head, the north-east promontory, there is a *sudden* retardation of the wave, arising from the narrow channel between Fair Head and the Mull of Cantire, which is contracted still further by Rachlin Island.

This is further confirmed by the survey of that coast recently executed by Captain MUDGE; by which it appears that the tide time at the Giant's Causeway, a few miles to the west of this point, is 6^h; while at Torr Point, a little to the east of Fair Head, it is $9^h\ 40^m$. In going to the south, Captain MUDGE gives 10^h as the time for Glenarm, $10^h\ 5^m$ for Belfast, and $10^h\ 30^m$ for Strangford, agreeing upon the whole pretty well with our previous data.

The suddenness of the change of the tide hour in passing round Fair Head is made more remarkable by the statement which Captain MUDGE gives of the tide in Ballycastle Bay, *immediately* to the west of Fair Head; he states for this place $5^h\ 50^m$, which is *earlier* than the tide at the Giant's Causeway to the westward.

This is in itself very extraordinary, and is opposed by the authority of the Survey published by Captain HUDDART (1790), who gives 7^h as the tide-time in Ballycastle Bay. And I can hardly think Captain MUDGE's statement consistent with the time, 8^h, which he gives for Church Bay, on the opposite shore of the strait between Rachlin Island and the main land; a strait not above five miles across, including the bays. If both Captain MUDGE's statements are exact, the water must, for two hours ten minutes, be rising on one shore of this narrow channel, while it is falling on the other; an occurrence which seems scarcely possible.

It is to be observed, that the tide here is small (only four feet at springs); so that accuracy in observing the time of high water must be difficult to obtain. And as the currents between the island and the shore are very strong, it is possible that the change of level due to the shifting of currents may mask the regular tide so as to displace the time of high water by several hours.

Taking Captain **Huddart**'s time for this point, it appears that for two hours forty minutes the water is falling in Ballycastle Bay within the Isle of Rachlin, while it is rising at Torr Point, which is beyond the Isle to the east. There must, therefore, be a strong current from the former to the latter of these points, which forms what is called the Race of Rachlin.

The retardation of the tide wave on the outside coast of Rachlin Island is probably not so abrupt as it is in the strait: still, however, it must be rapid; and for reasons of the same kind as those just mentioned, we shall have a strong tide current between the island and the Mull of Cantire: this is called the Race of Skerinoe.

Tides on the West Coast of Scotland.

The west coast of Scotland is so broken with promontories and islands, that the tides must be for the greater part *bay* tides; and it would introduce confusion to attempt to trace them in a general survey like the present. I shall therefore only consider the course of the main wave.

By Captain **Huddart**'s map it appears, that the 5^h line (or for *Greenwich time* the cotidal line of $5^h\ 30^m$,) passes near Tory Island. The same line, it appears, bends towards the shore so as to approach Icolmkill or Mull, for which also 5^h is marked. To the S.E. of this line, as at Isla, &c., the tides are later. The southern extremity of the range called the Western Isles is marked $5^h\ 30^m$, which is also the time given for the strait between North Uish and Harris. (Mr. **Lubbock** gives $6^h\ 30^m$.) Inside of the Lewis we have 6^h, both on the side of the island and the main land; and further north in the Minsh, near Cape Wrath, we have 7^h. **Norie** gives $8^h\ 15^m$, which, like his other statements for this western coast, is considerably larger than our authorities give.

For the Orkney Isles I find the following statements in **Norie**'s Sailing Directions for the East Coast of England and Scotland, p. 55.

	h	m
South Ronaldsha	9	0
Stromness, Wells, and Westra	9	0
Foul Island	9	30
Fair Island	10	0
Brassa Sound	9	45
East side of Sanda and North Ronaldsha	9	45
North Ronaldsha Frith	10	45
East side of Shetlands	9	30
Duncansby Head	8	15

It is tolerably evident from this, that the 9^h tide line nearly (Orkney time,) is the one belonging to these islands, where the wave is not retarded by narrow channels.

Tides of the Arctic Ocean.

The main tide-wave which we have been following, after reaching the Orkneys, will, I conceive, move forwards in the sea of which the shores of Norway and Siberia form one side, and those of Greenland and America the other. It will here meet in succession with the islands of Iceland and Spitzbergen[*]; it will pass the pole of the earth, and will finally end its course on the shores in the neighbourhood of Behring's Straits. Perhaps it may propagate its influence through the Straits, and modify the tides of the North Pacific.

But a branch tide is sent off from this main tide into the German Ocean: this, entering between the Orkneys and the coast of Norway, brings the tide to the east coast of England, the coast of Holland, Denmark, and Germany. We shall now trace this tide.

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[*] I add the following statements, which refer to the further course of the tide.

Bergen, lat. 60° 24′	1^h	30^m	NORIE.
Drontheim, lat. 63° 26′	2	15	——
Hammerfest, lat. 70° 40′	1	10	——
North Cape, lat. 71° 10′	3	44	——
Sweetnose (Lapland), lat. 68° 10′	8	30	——
Isle Kilduin (Lapland), lat. 69° 10′	7	30	LALANDE, p. 340.
Archangel	6	0	———, pp. 272 & 340.
Patrix Fiord (Iceland), lat. 65° 36′	6	0	———, p. 340.
Hakluyt's Head (Spitzbergen)	1	30	beginning of tide. PHIPPS, pp. 44 & 67.
Magdalen Bay	1	30	PHIPPS, p. 30.
Moffen Isle, 25th July 1773, low water at	11	0	——, p. 50.

Tides on the East Coast of Britain.

Taking for our authority NORIE's Sailing Directions for the East Coast. we find the following tide-hours (p. 38):

	h	m
Duncansby Head	8	15
Sinclair's Bay	9	0
Frith of Tain	11	0
From Bamff to Cromarty	11	45
Inverness	12	0
Buchanness	12	0

It appears that Inverness has a bay tide, a point of divergence occurring on the shore of Bamff.

By comparing the time given for Duncansby Head with that given for the Orkneys, and with that for Sinclair's Bay, which is a very little to the south of it, we should be led to think it earlier than the true time.

We have also (p. 32) the following tide-hours:

	h	m
Newburgh	12	30
Aberdeen	12	45
Montrose and Stonehaven	1	30
Tay Bar	1	45
Dundee	2	15
St. Andrew's	2	0
Fifeness	1	30
Leith	2	20
Dunbar	1	30

The places in Italics are affected by the retardation produced by the Friths of Tay and Forth. Perhaps Montrose is partly affected by this cause.

We proceed southwards, and find (pp. 17 and 2),

	h	m
Berwick and Eymouth	2	15
Holy Island	2	30
Fern Island	2	40
Blyth and Coquet Island	2	45
Tynemouth Bar	2	50

	h	m	
Sunderland	3	0	
Hartlepool, mouth of the Tees, and Whitby	3	30	
Scarborough	4	15	
Flamborough Head	4	30	
Bridlington Bay	4	30	
Spurn Point	5	15	
Sandhale	6	0	
Hull Road	6	15	
Dudgeon Shoal (at the mouth of the Wash)	6	0	
Lynn Well	6	30	
Cromer	7	0	NORIE's Epitome.
Lowestoff Roads	8	55	———
———, in shore	10	38	———
Harwich	11	30	———

As this time agrees with that at the North Foreland nearly, we may suppose the tide-wave to extend from one point to the other, and the tide which runs up the Thames from this point is a river-tide.

Tides on the remaining Coasts of the German Ocean.

We have the time at the Naze of Norway given as $11^h\ 15^m$, which corresponds with that on the shore of Caithness, so that the tide-wave would at first appear to extend directly across the northern opening of this sea. We find, however, that this cannot be the course of the cotidal lines, for we have 11^h for the hour at Heligoland, and hours a little later for the adjacent coasts; namely, (Sailing Directions for Heligoland, p. 8; and NORIE,)

	h	m
Scaw (north point of Jutland)	12	0
Hoorn (west coast of Denmark)	12	0
Island of Heligoland	11	0
Red Buoy (entrance of the Elbe)	12	0
Cuxhaven	1	0
Island of Wrangeroog (mouth of the Weser)	12	0
Island of Borkhum (mouth of the Ems)	11	30
Emden	12	0

Also, proceeding to the coast of Holland, we have (Directions for the North Sea,) these tide-hours:

	h	m
Island of Ameland	10	30
Vlie Passage	9	0
Texel (entrance to)	6	45
Texel Road	7	45
Camperdown	4	30
Hook of Holland	3	0
Briel (mouth of the Maes)	3	0
Browershaven (East Schelde)	2	0
Veer	1	20
Flushing (West Schelde)	1	20
Blankenberg	12	20
Ostend (as before)	12	20

It appears quite clear from this list, that the tide-wave on this coast runs to the east; and this is also the case with the tide currents; the general flood-stream running N.E., and the ebb S.W. (Sailing Directions, p. 55.)

We seem to be led therefore to the remarkable conclusion, that a tide-wave which reaches almost or quite to the shores of Denmark, proceeds through the narrow opening of the Straits of Dover, though the main part of the German Ocean is occupied with the tide-wave which enters by the north; and that the tide-wave on the opposite shores of England and Holland runs at the same moment in opposite directions.

In consequence of this state of things, it is very difficult to assign the form and motion of the tide-wave in the middle parts of this ocean. The tides there will be *tides of interference*; (see Sect. 1.), and the undulations of the surface may be stationary instead of successive undulations, and may be defined by spaces instead of lines. Numerous observations at a distance from shore will be requisite to determine the circumstances of these undulations.

On the Leman and Ower shoals, $6^h\ 30^m$ or 7^h is the tide-hour. The flood comes from the north upon the whole, though its direction varies through a certain angle during the flow; whether the flood sets north or south off the coast of Denmark, I do not find stated. Captain ANDERSON (Phil. Trans.

1819, p. 226,) says, "The opposite tides which meet in the North Sea do not meet in a line directly across any part of it, but in a diagonal line extending from the Kentish Knock to the entrance of the Sleeve;" which seems to imply that the tide on the Danish coast comes from the S.W. He adds, "In fact, there is hardly any tide observable between the Hoorn reef and the entrance of the Sleeve."

The 7^h tide line on the Ower shoal is further back than the same line on the shore; which, being contrary to the course of single tides, appears to indicate there a tide of interference; the absence of tide on the coast of Jutland is also probably the result of interference, for the tides at Heligoland are said to rise 9 feet (NORIE). If we suppose the southern and northern tides to reach this space at an interval of 6 hours,—for instance, if the tide-hours for these tides be respectively 3^h and 9^h,—the tide will be altogether obliterated by their combination.

We may therefore account for several of the facts, by supposing the 9^h tide-line to advance from the north in a convex form, so as to approach this part; and the southern tide to move so as to produce the lines 12^h, 1^h, 2^h, 3^h on the coast of Denmark, if it were single. We may thus suppose in the middle of the ocean *a space* which has its tide at 6^h nearly, by the interference of the tides

at 4^h 5^h 6^h 7^h 8^h from the north,
and 3^h 7^h 6^h 5^h 4^h from the south.

A circumstance in the rise of the tide in this part appears to confirm the opinion that it is a tide of interference. "A singular peculiarity in the tide about the Ower was observed;—there was no sensible *rise* in the tide until 3 hours after low water; and when the ebb stream was nearly done, a sudden rise of 5 or 6 feet took place; so that nearly the whole rise of the tide occurs in the last 3 hours of it." (Captain HEWETT on the Leman and Ower Shoals.)

Still it is scarcely possible that a tide so considerable as that on the north coast of Germany should be entirely produced by the undulation propagated through the Straits of Dover. By looking at the map, it will be seen that if we draw a line from Norfolk on one side to the Texel on the other, we cut off a portion of the sea extending to Dover, which may be considered as merely an inlet of the German Ocean; and the tides of the main expanse might be expected to be nearly independent of those of this inlet. If this inlet were closed, the tide-wave would pass from the coast of Norfolk to that of Holland, the hour being 6^h in Lynn Wash, and 7^h at the Texel; proceeding regularly from Caithness to Heligoland. This is a view of the origin of the tides of the German Ocean, different from the former one. At present it appears to be difficult to decide which is the more correct.

On the Tides of the South Atlantic.

It will be recollected that the tide which entered the South Atlantic found a point of divergence at or near Cape Frio on the coast of Brazil. We have traced the northern branch of the tide to the end of its career, and have particularly examined its movement on our own and the neighbouring coasts. We shall now endeavour to trace the southern branch of this tide.

Proceeding along the coast southwards from Cape Frio, we find Rio Janeiro at a small distance, for which we have the establishment $2^h\ 40^m$ (Norie), or $2^h\ 45^m$ Roussin; and adding $2^h\ 52^m$ for longitude to the latter, the tide-hour, Greenwich time, is $5^h\ 37^m$: Mr. Lubbock gives $5^h\ 15^m$.

The island of Santa Catharina, in south latitude 27°, is the next point. Norie gives $2^h\ 30^m$; Roussin $2^h\ 45^m$, or Greenwich time $5^h\ 57^m$, which agrees nearly with Mr. Lubbock.

In the Sailing Directions for the coast of Brazil which I have seen, I do not find any account of the tide-hours at any point of the coast south of Santa Catharina*. Our next materials are Captain King's survey of Patagonia.

In Captain King's Sailing Directions for the Coast of Eastern and Western Patagonia, we have for Port St. Elena, in latitude 44° 30′, the time of high water at full and change 4 o'clock. The longitude is $4^h\ 20^m$ W. Hence the tide-hour, Greenwich time, is $8^h\ 20^m$.

It would at first appear from this, that the tide-wave employs less than 4 hours in passing from Cape Frio to Port St. Elena, running southwards along the shore: but we shall find various circumstances which will not allow us to consider this as the true motion of the tide-wave. It appears from Captain King's observations (Directions, p. 17), that along the whole of the coast in the neighbourhood of Port St. Elena, the flood tide sets to the *northward*, while off the shores of Brazil the flood sets to the *southward*, (NORIE's Sailing Directions for Brazil, Part I. p. 45). And though it is not universally necessary that the cotidal lines should move in the same direction as the flood stream, there can hardly be a discrepancy between these directions on such a scale as the southward motion of the tide-wave to St. Elena would seem to imply. We are therefore compelled to suppose that somewhere on the coast to the north of St. Elena there is *a point of convergence of cotidal lines*, at which point they are later than either to the north or to the south of it.

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* The tide at Monte Video is said to be at noon (Forte's Remark Book), and at Blanco Bay at 6^h; it is easy to accommodate the cotidal lines to the latter datum; the former is probably affected by the influence of the river.

This view is confirmed by our finding that on the coast of Patagonia to the south of Port St. Elena, the cotidal line appears undoubtedly to travel northwards. Thus Captain KING, p. 17, makes the following statement, taking points from Cape Virgins at the eastern entrance of the Strait of Magelhaens, in order, going northwards.

	Latitude.			Tide-hour.			Feet.
	°	′		h	m		
Cape Virgins	52	21 S.	about	8	0		
Gallegos River	51	43		8	30	rise	46
Cape Fair Weather	51	33		9	0		28
Coy Inlet			between	9 and 10^h			
Santa Cruz	50	17		10	15		33
Port St. Julian	49	8		10	34		38
Sea Bear Bay	47	56		12	45		20
Port Desire	47	45		1	0		21½
Port St. Elena	44	30		4	"in the afternoon."		

Hence it appears that the tide travels from Cape Virgins northward to Port St. Elena in about eight hours: and as, from the form of the coast, we cannot doubt that the tide must reach Cape Frio before it penetrates the bay formed by the east coast of Patagonia, it appears that the tide at Port St. Elena is not four hours, but sixteen hours, after that at Cape Frio.

The tide which comes from the southward is one of great magnitude, the spring tide heights ranging from twenty to thirty feet along the coast, and attaining forty-six feet at the Gallegos River. The tide which comes from the north is much smaller; it is stated at five feet at Cape Frio, four feet at Rio, six feet at Santa Catharina. In the River Plata the tides do not rise or fall more than five or six feet (Captain Heywood in Norie's Brazil, p. 47). Hence the tides to the south of the River Plata must be principally produced by the wave which comes from the south. Probably Cape Corrientes, in latitude 38°, may be taken as the southernmost headland reached by the tide from the north. We have no evidence (so far as I am aware,) what time the northern tide employs in reaching this part of the shore. I shall suppose Cape Corrientes to be the point at which the tide is simultaneous with that at Cape Virgins; and between these points the cotidal lines will advance from both extremities to an intermediate *point of convergence of cotidal lines,* which may perhaps be at the heads of the Gulf of St. George, or of St. Antonio, in latitude 41° and 46°.

Though this determination of the form and course of the cotidal lines of this coast may be considered somewhat hazardous, it derives considerable confirmation from what takes place on another coast somewhat similarly circumstanced, that of North America; besides being, so far as I can perceive, the only way in which the observations can be made consistent with each other.

The cotidal lines which are drawn in the chart may appear very much *thronged;* but it may be observed that we have good evidence that they must be as close to each other as they are there drawn, because there are eight of them which reach the shore between Cape Virgins and Port St. Elena, about 8° of latitude. We have also from Purdy (E. M. p. 59,) the following statement of the times of high water at various points of the Falkland Islands:

	h	m
Berkeley Sound	5	0
North entrance of Falkland Sound	6	30
Tamer Harbour	7	0
Pebble Sound	8	30
Saunders Island	7	30
Jason's Isles	8	0
Swan Islands	8	30
Port Stephens	8	0
Port Albemarle	7	45

Swan Islands and Port Stephens are on the western side of this land, and therefore retarded; and as 4^h W. is the longitude of this land, the cotidal line of 9^h appears to pass near the eastern part of these islands; while at Cape Virgins, in longitude 4^h 33^m W., we have the line of 12^h 30^m.

The cotidal line at the eastern extremity of Tierra del Fuego is that of 8^h 35^m Greenwich time; for in Good Success Bay, latitude 54° 48′ S., longitude 4^h 20^m W., the time as stated by Captain KING is 4^h 15^m.

The time of high water at South Georgia (COOK in PURDY'S E. M. p. 36,) is 11^h; and as the longitude of this land is about 2^h 24^m, the cotidal line of 1^h 30^m passes near it.

On the Tides of the Pacific.

1. *Western coast of America.*

From Cape Pillar, at the western extremity of the Strait of Magalhaens, the shore of Tierra del Fuego runs E.S.E. to Cape Horn, and then E.N.E. to Strait le Maire and Staaten Island. Along this coast the tidal wave travels to the *eastward*. Thus, according to Captain KING (Sailing Directions, p. 96, and Table, pp. 13, 14,) at Cape Pillar it is high water at 1^h on the days of full and change; at York Minster, 5° of longitude to the east, it is at 3^h; at Cape Horn, 3° further east, it is at $3\frac{1}{2}^h$; in Good Success Bay, in Strait le Maire, the hour is 4; on the east side of Strait le Maire it is 5^h, (p. 100). As there is a constant easterly current off Cape Horn (p. 102), the ebb may be supposed to be produced by a retardation only of this motion: nearer Cape Pillar, the tide current came from the N.W. (p. 96); the tides here are about four feet.

The motion of the cotidal lines upon a large scale is undoubtedly from east to west: and it is certainly very remarkable that in this part of the ocean where there appears to be nothing to interrupt the westerly motion, the lines should travel in an opposite direction. This however is only a partial phenomenon, like the convergencies on various coasts which we have already had to notice; for a little further to the east and north, at Staaten Island, and on the coast north of Cape Diego (at Strait le Maire), the tides set to the north and west (p. 106).

In order to determine the motion of the tide-wave on the west coast of America, we have the following data:

	Latitude.	Tide-hour.	Green. Time.	
	° ′	h m	h m	
Cape Pillar	52 46 S.	1 0	6 0	
San Carlos de Chiloe	41 52	11 30	4 26	6ft. King's Table, p. 15.
		12 30	......	12ft. Heron R. B.
Valdivia	39 50	11 30	4 24 4 45	Norie; Purdy, E. M. p. 59. Lubbock.
Conception	36 49	10 0	2 50	Malaspina, p. 127.
		8 30	......	Thetis R. B.
		9 45	......	Thetis R. B.
Conception	......	[1 30]	......	Forte R. B., and Perouse in Purdy E. M. p. 204.
Talcahuana (harbour of Conception)	......	10 0	......	Bauza, MS.
Talcahuana	......	[3 20]	......	Beechey, p. 645.
Valparaiso	33 2	9 25	2 10	Malaspina, p. 127.
		9 0		Tribune R. B.
Coquimbo	29 54	9 0	1 45	Tribune R. B.
		9 40		Forte R. B.
Coquimbo	......	[2 37]	......	Malaspina, p. 127.
Copiapo	27 12	[2 30]	......	Lubbock.
Callao	12 4	6 15	11 23	Malaspina, p. 127.
Guayaquil	2 12	7 19	12 42	Ibid.
Galapagos	1 21			
———— Charles's Bay	1 0	2 0	8 1	
———— Chatham Isle	1 0	3 30	......	Purdy, E. M. p. 51.
Cocos	5 34 N.	2 10	......	——, E. M. p. 50, Vancouver.
		4 0	......	——, E. M. p. 47, Colnett.
Panama	8 57	2 30	7 47	Lloyd in Phil. Trans. 1830.
		2 0	......	Foster (S. A. P. 2, p. 91.)
Realejo	12 30	2 43	8 31	Malaspina, p. 127.
Acapulco	10 50	1 19	7 59	Ibid. tide very small.

Among the different hours given for Conception, there can be little doubt that $1^h\ 30^m$ is erroneous, whether the error arise from the confusion of high water with slack water, or from whatever other cause; the superior weight of evidence and the times of neighbouring places show that the time here must be about 9^h, though Lieutenant Beechey's statement is so different. Similar considerations induce us to reject $2^h\ 37^m$ for Coquimbo*. And the causes, whatever they are, which have given rise to these erroneous statements for Conception and Coquimbo, have probably given rise to the statement for Copiapo, which Mr. Lubbock has recorded, and which is irreconcilable with the course of the tides on the rest of the coast.

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* On referring to the original MS. of the observations made in Malaspina's voyage, which Mr. Bauza has obligingly shown me, I find that while the time of tide at the other places is termed "pleamar," at Coquimbo it is stated as "hora en que cambio."

Omitting, then, the times thus condemned, we perceive that there are, with slight irregularities only, hours perpetually earlier and earlier as we go further north all the way to Acapulco, and that therefore the tide-wave travels along this coast from north to south, employing about twelve hours in its motion from Acapulco to the Straits of Magalhaens. In confirmation of this view it may be observed, that according to Captain Colnett (Purdy, E. M. p. 216), at the island of Quibo, westward of the Bay of Panama, the flood comes from the north at full and change, "flowing seven hours and ebbing five:" and along the whole coast of Peru and Chili, the tide sets to the southward (Lalande, p. 291).

At the Gallapagos, according to Purdy (E. M. p. 59), the flood is from the east; as it also is at the Cocos (p. 50).

The Gulf of Panama has nearly the same tide-time as the Gallapagos and other islands opposite to it in the open ocean. When we consider the deep inbend of the shore of this bay, and the great height of the tides which take place in it (eighteen or twenty feet at St. Michael's Bay on the eastern side of the Gulf, Lalande, p. 293), it is difficult to suppose that the tide-wave occupies so short a time in travelling up the Gulf as this statement would imply. If, therefore, the hours given for Cocos Island, Charles's Island, and Chatham Island are nearly right, it is more probable that the tide-wave employs twelve hours in passing from the neighbourhood of the Gallapagos to the extremity of the Gulf. The tide-line will travel slowly in the narrower part of the bay, especially if it be shallow; and I shall adopt this as the most probable view of the cotidal lines, till we have more numerous and more certain observations.

As we proceed further north we have evidence that the tide-line is now travelling north: we find

	Latitude.		Tide-hour.		
	°	′	h	m	
San Blas	21	32	8	5	Mem. on S. Am. P. 2. p. 100.
Mazatlan	23	0	9	41	Beechey, p. 661.
			9	50	———, p. 661.
San Diego	32	42	9	0	
San Carlos de Monterey	36	36	10	0	Mem. on S. Am.
			9	42	Beechey, p. 655.
			8	0	Malaspina.
San Francisco	37	48	10	52	———
Colombia R.	46	19	1	30	
Nootka Sound	49	34	0	20	Norie.

The tide at the Colombia is probably retarded by the inlet.

It would seem therefore that there must be a point of divergence somewhere in the neighbourhood of Acapulco (lat. 16° 50′). The tide at Acapulco is difficult to reconcile with the rest; and as the rise is very small, we may perhaps venture to suppose the time considerably erroneous.

2. *Central parts of the Pacific.*

I fear it is at present impossible to draw the cotidal lines, with any strong probability of their being right, between the coasts of Chili and New Zealand. We should certainly expect them to travel from east to west; and as there must, as we should conceive, be twenty-four different cotidal lines in going round the globe, there must, it would seem, be a considerable number of them in this wide ocean. Our knowledge of the tides in this part of the world is very scanty. The extreme smallness of the height of the tide makes it difficult to observe it with certainty, and allows it in many cases (for instance, in the Sandwich Isles,) to be completely masked by the constant daily effect of the land and sea breezes.

Proceeding westward across the Pacific, we have the following statements:

	Long. W.		Tide-hour.		Green. Time.		
	h.	m	h	m	h	m	
Gallapagos Island, Charles's Bay.	6	1	2	0	8	1	7 to 8 feet.
Easter Island	7	18	2	0	9	18	Norie.
Gambier's Group	9	0	1	50	10	50	Beechey, p. 646.
Lagoon Island	9	18	0	30	9	48	Cook, Phil. Trans. 1772.
			11	15	8	33	Lalande.
Society Islands.							
Otaheite	9	58	0	15	10	13	———
Ulietea	10	6	11	30	9	36	———
Huaheine (Owharree Bay)	10	4	11	50	9	54	Norie.

The times for Otaheite and Ulietea are calculated (Lalande, p. 298,) from observations made for several days. The tides in this group of islands are extremely small; at Otaheite, for instance, only eleven inches. This circumstance is attributed, by Mr. Wales, to the banks of coral which surround these islands, and which leave only narrow openings for the tide to pass in and out: the same cause will probably affect the time of high water, but it is difficult to say how much. Indeed, we may doubt whether this small rise really exhibits the effect of the lunisolar tide. In some places other diurnal influences completely mask the effect of the attraction of the sun and moon. Thus, Captain Beechey observes, that at Papiate, one of the Society Islands, it is high water *every day* at half an hour past noon, and low water at six in the evening.

Lieutenant Malden (Lord Byron's Voyage,) gives a similar account of the tides at Owhyhee.

The establishment for Resolution Bay in the Marquesas Islands (long. W. $9^h\ 15^m$) is stated at $2^h\ 30^m$, which seems to show some irregular form of the tide-wave. The deficiency of materials, however, with regard to this part of the sea, is so great, that I shall not attempt to draw the cotidal lines in the Pacific north of the equator.

3. *Western parts of the Pacific.*

When we arrive at the more extensive and closer islands on the west side of the Pacific, we can trace the course of the tides with a little more definiteness. Captain Cook has stated the times for several points on the coast of New Zealand (Phil. Trans. 1772.). At Tolaga Bay, near the most easterly point of these islands, the time is 6^h. In proceeding to Mercury Bay and the Bay of Islands, on the N.E. coast, it becomes $7^h\ 30^m$ and 8^h respectively. Again, in proceeding southward from Tolaga Bay, we have also a retardation. At Queen Charlotte's Sound and Admiralty Sound, in Cook's Strait which separates the two islands, it is $9^h\ 30^m$ and 10^h, the Strait producing a considerable retardation. At Dusky Bay, at the southern point of the land, the time is $10^h\ 57^m$. Hence it is clear that the 6^h cotidal line meets the coast near Tolaga Bay, and there forms a point of divergence, the tides passing round the ends of the islands to the north and south, occupying three or four hours in turning each extremity.

The tide which was on the coast of Patagonia at five o'clock, and on the coast of New Zealand at six, must have employed thirteen hours in this passage; but, as it is moving to the southward and eastward in the former case, and to the westward in the latter, the cotidal lines must somewhere have a vertex, which is probably turned towards the north.

The Friendly Islands, which are nearly to the north of New Zealand, receive the tide-wave about the same time as that coast, as appears by the following data:

	Longitude.		Tide-hour.		Green. Time.		
	h	m	h	m	h	m	
New Zealand.							
Tolaga Bay	11	53 E.	6	0	6	7	
Friendly Islands.							
Annamooka	11	40 W.	6	0	6	20	
Tongataboo	11	41	6	50	7	19	
Eooa	11	40	7	0	7	20	
Wallis's Island	11	44	5	0	4	44	Zebra R. B.

Hence it appears probable that the 6^h and 7^h cotidal lines extend nearly north and south as far as the equator; and the following data, referring to places which lie in the intermediate space, but a little more to the west, agree with this view.

	Longitude. h m	Tide-hour. h m	Green. Time. h m	
Norfolk Island..........	11 12 E.	7 45	8 33	Norie, Lubbock.
New Caledonia.				
Balada Harbour	10 58	6 30	7 32	———
Pudyona	....	6 30		———
New Hebrides.				
Tanna, Port Resolution ..	11 19	5 45 [3 0]	6 26	Lalande, p. 298. Norie.

The tide occupies an hour or two in passing to the other coast of New Caledonia around the north or south promontories; at Port St. Vincent, on the west shore, the hour is 8^h 10^m (Norie).

Three or four degrees south of the south point of New Zealand are Lord Auckland's Isles, discovered by Capt. Bristow in 1806. Mr. Lubbock states the tide hour at 11^h 30^m, which, if true, indicates that the cotidal lines here are brought near each other even at a considerable distance from the coast of New Zealand. It is difficult to conceive that the tide at a detached island should be half an hour later than at Dusky Bay, which is not only more to the west, but has the tide brought to it by a wave which travels along an extensive retarding coast, round a long promontory, and through a large portion of a whole circumference.

On the Tides of the Coasts of Australia.

For the tides of the Australian coasts our materials are more abundant, Capt. Flinders and Capt. King having pretty fully examined these seas. The most easterly part of the shore, from lat. 24° to 35° S., appears to receive the tide earliest. Thus we have,

	Lat. ° ′	Tide-hour. h m	
Bustard Bay	24 30	8 0	Cook, Phil. Trans. 1772.
Hervey's Bay	24 40	8 0	Flinders (ii. 11).
Botany Bay	34 0	8 0	Cook, Flinders.

Various points within this district have the tide later, but they are places where a promontory or island intervenes between them and the open sea. Thus,

	Lat.		Tide-hour.		
	°	'	h	m	
Port Jackson	33	50 S.	8	15	FLINDERS.
Rodd's Bay	23	59	8	30	KING (ii. 261).
Shoal's Haven	34	45	8	30	FLINDERS.
Moreton Bay	27	10	9	30	———

In proceeding to the northwards, we find that the hour becomes perceptibly later. We have,

	Lat.		Tide-hour.		
	°	'	h	m	
Port Curtis	23	52 S.	8 to 9	0	FLINDERS.
Keppel Bay	23	8	9	30	———
Port Bowen	22	28	10	0	———
Strong-tide Passage			10	0	———
Shoal-water Bay			10	30	———
Thirsty Sound	22	6	10	45	———
Broad Sound			11	0	———
Percy Islands	21	19	8	0	———
Cape Hilsborough	20	53			
Cumberland Isles			11	0	———
Endeavour River	15	27	9	30	———, COOK.
Princess Charlotte's Bay			8	0	KING (ii. 281).
Endeavour's Strait	10	37	1	30	———
Murray's Island in Torres' Strait	9	55	10	30	FLINDERS.

The earliness of the tide at Murray's Island may be accounted for, as the island is at some distance from the main shore, and the tide-line will, as usual, be convex. The tides at Percy Islands are less consistent with the rest. Capt. FLINDERS (ii. 82.) gives the time as observed by Capt. FOWLER, and expresses his surprise at the difference of three hours from Broad Sound. Capt. KING (ii. 263.) observed the tide on two successive days to be about 10 hours, and 11 hours after the moon's transit. This, even corrected for the semi-menstrual inequality (see Sect. 2), would give the hour much nearer that on the shore.

The earliness of the time given by Capt. KING for Endeavour River, compared with the times at adjacent places, and its differing both from FLINDERS and COOK, are grounds which authorize us to suspect it.

Hence we collect that the tide-wave occupies about 3 hours in moving northwards along the coast from lat. 30° to Torres' Straits between New Holland and New Guinea, and reaches the points within the Straits westward later still. The cotidal line which meets the coast at 8^h, in long. $10^h 15^m$ E., is that of $9^h 45^m$; and the one at the Straits, in long. $9^h 30^m$ E., is that of 1^h. This agrees with Capt. King's statement (ii. 259), that in this part the flood sets N.W.

Beyond this point the general run of the coast is to the west; and we can trace the course of the tide by means of Capt. King's Survey. We have the following data.

	Long. E. h m	Tide-hour h m	
Endeavour Straits	9 25	1 30	Cook.
Liverpool River and Goulburn Island		6 0	(King, ii. 309.)
Alligator River, in Van Diemen's Gulf (a gulf with narrow entrance)	8 48	8 15	
Port Cockburn	8 42		
St. Asaph's Bay		5 45	(ii. 237.)
King's Cove		5 15	(ibid.)
Vansittart Bay	8 22	9 15	(ii. 324).
Montague Sound		12 0	(ibid.)
Careening Bay	8 20	12 0	(ibid.)
Prince Regent's River		12 0	(ibid.)
Roebuck Bay	8 8	30 feet,	King, Dampier.

At Roebuck Bay the tides flow from the westward (King, ii. 355); so that probably the progression would no longer go on. I find, however, no further observations on the tides in Capt. King's Survey, though the land extends to $7^h 32^m$ long. E.

The time at Careening Bay shows that we have there the cotidal line of $3^h 40^m$.

Returning to the east coast of Australia, and proceeding southwards from latitude 35°, we find the times to become later, so that there is *a point of divergence* on the eastern coast.

The following tide comes from the east (Flinders, &c., Plate VII.).

	Lat. S. ° ′	Tide-hour h m
Bay at the entrance of Banks's Strait	40 45	9 0

After this the shore runs to the west, and we have the following tides:

	Long. E.	Tide-hour.	
	h m	h m	
Corner Inlet (with a narrow entrance)	9 45	11 15	BASS, in FLINDERS's Chart.
Port Dalrymple (north side of Van Diemen's Land)	9 47	11 45	FLINDERS's Chart.
Head of Spencer's Gulf (a deep inlet)	9 11	2 15	————
Nepean Bay, in Kangaroo Island (with a narrow passage to the east)	9 11	4 0	————
Thorny Passage	9 4	12 0	————

The first and the last of these places may be looked upon as fitted to give the position of the cotidal lines on the coasts; the others are probably too much affected by the retardation of the inlet. Therefore the 3^h cotidal line passes near the point of the coast which is in 9^h longitude east.

I do not find any information concerning the tides of any part of Australia to the west of this. From the comparative narrowness of the passage to the north, it is almost certain that these tides must come from the southern side of the continent; and we have found reason, as already stated, for believing that these southern tides extend to a portion of the N.W. coast.

On the Tides of the Indian Ocean.

If we examine Mr. LUBBOCK's Chart, it will appear that the cotidal lines of 11^h, 12^h, 1^h, (Greenwich time,) run along the skirts of the great open space of the Indian Ocean. Thus we have on the south coast of Sumatra,

	Long. E.	Tide-hour.	Green. Time.	
	h m	h m	h m	
Bencoolen*	6 50	5 50	11 0	
Cracatoa Island, Strait of Sunda	7 2	7 0	11 58	NORIE.
Acheen, west end of Sumatra round the point	6 22	9 0	2 38	———
Ceylon: Trincomalee	5 25	6 0	0 35	———
Maldive (King's Island)	4 52	2 0	9 8	HORSBURGH, 306.
Chagos Island (Solomon's Island)	4 50	1 0	9 10	NORIE.
		1 30	8 45	LUBBOCK.
Mahee Islands (6 feet)	3 42	5 30	1 48	HORSBURGH, 126.
Amirante Islands (Remire) (9 feet)	3 36	3 30	11 54	————, 127.
African Islands (8 feet)	3 37	9 39	6 2	————, 127.

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* NORIE gives for the time at Fort Marlborough, Bencoolen, 0^h 0^m, which does not agree with any of the neighbouring parts.

These three statements of Horsburgh are so irreconcilable with each other (for the islands are small and near together,) and with the general course of the cotidal lines, that I shall reject them. Mr. Lubbock gives from other authority 12^{h} for the Greenwich time at the Mahee Islands, 12^{h} 15^{m} at the Amirante Islands, and 12^{h} at St. Laurent, a little to the south. This will agree with the general course of the lines.

We have also,

	Longitude. h m	H. W. Time. h m	Green. Time. h m	
Roderigue Island (6 feet)	4 13	12 30	8 17	Norie.
		12 45	8 32	Horsburgh, 114.
		3 13	11 0	Lubbock.
Mauritius (Port Louis)	3 50	12 30	8 40	Norie.
Bourbon	......	1 5	9 15	Lubbock.

It will be seen that these data are extremely discordant. A coast so extensive as that of Madagascar may be expected to offer more consistency. We have the following data in Norie.

East coast of Madagascar:

	Longitude. h m	H. W. Time. h m	Green. Time. h m
Samatava Point	3 18	4 18	1 0
Fort Dauphin	3 8	4 30	1 22

West coast of Madagascar:

	Longitude. h m	H. W. Time. h m	Green. Time. h m
St. Augustin's Bay	2 54	4 30	1 36
Makumba Island	3 3	4 45	1 42
Majambo Bay	3 8	4 30	1 22
Minow Island	3 9	5 0	1 51
Luza River (entrance)	3 10	4 30	1 20
Passandava Bay	3 13	5 0	1 47

Mozambique Channel:

	Longitude. h m	H. W. Time. h m	Green. Time. h m	
Johanna Island	2 57	3 0	0 3	
Sofala	2 19	4 0		
Great Comoro	......	....	2 30	Lubbock.

We have also in the neighbourhood of the Cape of Good Hope,

	Long. E. h m	H. W. Time. h m	Green. Time. h m
Plettenburg Bay	1 34	3 10	1 36
Algoa Bay	1 42	3 20	1 38

It is obvious that the tide is at nearly the same time on the whole coast of the island, but a little later on the inner side; and as we cannot suppose these determinations to be all widely erroneous, we may suppose that the 1^{h} cotidal line passes near the eastern coast of Madagascar.

The time at Zanzibar on the coast of Africa (E. long. 2^h 37^m) is 4^h 45^m, which gives Greenwich time 2^h 8^m. Mr. Lubbock has 1^h 15^m. The lines may be accommodated to either, or to the mean*.

I shall, upon the preceding data, suppose the 12^h cotidal line to touch the coasts of Sumatra and of Ceylon, to descend southwards near the Mahees and between Mauritius and Madagascar, and to reach perhaps the meridian of the Cape of Good Hope. The 1^h line will of course be outside this, and will be inflected and broken by the various bays and islands which occupy the skirts of the Indian Ocean. Within the 12^h line, the 11^h, 10^h, 9^h run, in some measure following its course; the 9^h through the Maldives and Chagos Islands. Within this again will fall the lines of 8^h, 7^h, and 6^h. It would appear that the latter must pass near the islands of St. Paul and Amsterdam, and Kerguelen's Land; for we find these statements:

	Long. E.		H. W. Time.		Green. Time.		
	h	m	h	m	h	m	
St. Paul	5	9	11	0	5	51	Horsburgh, 83.
Christmas Harbour, (Kerguelen's Land)	4	36	10	0	5	24	Norie.

The 1^h, 2^h, 3^h, &c. cotidal lines near the south shore of New Holland are, as we have seen, much retarded by the shore. Hence at a distance from the coast they will bend forwards, and finally northwards and eastwards, so as to meet the wave which comes more slowly still through Torres Straits. From these considerations we may complete the course of these lines with some approximate probability.

In following the cotidal lines into narrower seas, as the Bay of Bengal and the Arabian Sea, they become closer, according to the general rule. And, according to Mr. Lubbock's chart, the tide which arrives at Madras at 4^h 15^m Greenwich time, is at Calcutta at 5^h, and reaches the coast of Aracan at 7^h.

It is difficult to reconcile Norie's statements concerning the Nicobar and Andaman Islands in the Bay of Bengal. He gives for the former 9^h 15^m, and for the latter, which are 5° to the north of the others, 4^h 30^m.

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* Mr. Horsburgh gives the following tide times in this neighbourhood.

P. 180.	Mongallon	3^h 45^m
181.	Quiloa	3 45
183.	Zanzibar	4 30
	Mombas	12 0
184.	Patta	4 30

In the Arabian Sea the tides also come from the south. The wave which is on the coast of Africa at the equator at 2^h Greenwich time, reaches the shore of Arabia some hours afterwards. We have,

	Latitude. ° ′	H. W. Time. h m	Green. Time. h m	
Socotora Island (at the entrance of the Sea of Bab-el-Mandel)	12 22 N.			
Cape Morebat	17 0	9 0	3 40	HORSBURGH, 220.
Mazeira Island	20 0	10 48	5 9	———, 221.
Ras al Gal (at the entrance of the Persian Gulf)	22 22			

On the Indian side of this sea at Kurachee, Kutch, Bombay, and Goa, the tide arrives nearly at the same time; that is, at 11^h, 11^h 30^m, and 12^h after noon, and at about 6^h 30^m Greenwich time; so that the tide wave must be here nearly parallel to the shore.

With such materials as we at present have, it would be useless to attempt to trace the tide further into the narrower seas, as the Red Sea and the Persian Gulf.

Many maps and charts of various portions of the coasts which border the Indian seas have been constructed from surveys made by direction of the East India Company. I have examined a collection of these, but have found very few notices of tides, and scarcely any for those parts which would be most decisive with regard to the form of our cotidal lines; as the south coasts of Ceylon, Sumatra, Java, and the detached islands and archipelagos.

On the Tides of Rivers.

When the tide-undulation reaches the mouth of a river, it enters in the manner which we have described for any branch tide. The line of the wave revolves round the promontory which it first reaches, of the two which form the opening of the river, and in this way soon meets the opposite promontory. After this it assumes a direction at right angles to the course of the river, and advances regularly up the stream. There are some circumstances which require notice in such tides.

Magnitude of River Tides.

If we trace the tide-hours from the sea on one side up the seaward part of the river, and down the other side to the sea again, we may consider the banks as a portion of the shore in which there is a point of convergence of tides; and the magnitude of the tides will be increased by this convergence, as happens in cases of convergence in general. This augmentation may amount to something very considerable, as in the case of the tides of the Wye at Chepstow, which rise sixty feet. If we consider these very high tides of convergence as a mechanical question, they may be accounted for by what is called "the principle of the conservation of force." When any quantity of matter is in motion, its motion is capable of carrying every particle of the mass to the height from which it must have fallen to acquire its velocity; but if the motion be employed in raising a smaller quantity of matter, it is capable of raising it to a height proportionally greater. In bays and channels which narrow considerably, the quantity of water raised in the narrow part is less than in the wider, and thus the rise in such cases is greater.

When the tide has reached the part of the river beyond which the width does not greatly vary, the wave advances up the river, but its height gradually diminishes by the various causes (friction, impact against obstacles, &c.) which absorb its "force." The great tide of the Wye at Chepstow is, in this way, extinguished before it reaches Tintern.

We may trace this extinction of the tide in the Thames by means of observations which Mr. Rennie has had made at various points. The following are means of two groups of four days in different parts of a lunation in January 1832.

Height of the tide at	ft.	in.	ft.	in.
London Dock	18	0		
Putney	10	2	8	0
Kew	7	1	4	8
Richmond	3	10	1	10
Teddington	1	4½	0	8½

In like manner in the Gironde we have first an increase of height by contraction, and then a diminution by the usual impediments. The following heights succeed each other in the course of this river. (Daussy, p. 80.)

Cordovan	7·28 feet (French).
Saint-Surin (3½ leagues within)	7·62

La Marechale (7 leagues within) . .	8·21 feet (French).
Isle of Patiras (opposite Blaye) . . .	8·41
Bordeaux	7·36

In the River of Amazons the tide is still sensible at two hundred leagues from the mouth of the river. (Lalande, p. 153.) It occupies several days in ascending to this point; and it has been calculated that there are eight tides travelling along the river at the same time at proper intervals.

In the River St. Lawrence the tides penetrate 432 miles up the main channel, to a point between Montreal and Quebec. (Stuart's America, i. 162.)

The Bore in Tide Rivers.

When the tide is made to rise greatly by the contraction of its channel in the manner just spoken of, the part of the water so affected may be abruptly terminated on the inland side, the depth and quantity of the water on that side not allowing the surface there to be immediately raised by means of transmitted pressure. A tide-wave thus rendered abrupt has a close analogy with the waves which curl over and break on a shelving shore. Such a tide-wave is called a *bore*, and in many places occurs in considerable magnitude; producing great noise by the large amount of intestine motion of its particles, and appearing to travel with great rapidity, though it in fact moves slower than the tide-wave under any other circumstances.

The *bore* which enters the Severn is nine feet high; that in the Creek of Fundy is said to be still higher. In the Garonne this phenomenon takes place near Bordeaux, and is called there *le Mascaret;* at Cayenne it is called *Barre;* it occurs in the River of Amazons, at the junction of the Arawary, having there a face twelve or fifteen feet high, and producing a noise which may be heard at two leagues distance; it is called by the Indians of the neighbourhood *Pororoca*. (Lalande, Art. 202.)

High and Low Water in Rivers.

In the sea, the times from high to low water, and from low to high water, are nearly equal; but in rivers these two intervals may be very different. It has appeared that tides *die* in travelling up a river; and it will be easily conceived that a small tide would travel a shorter distance than a large one before its extinction. Therefore, a rise of tide which occurs in the lower part of a river must have a certain magnitude, in order that it may affect a point in the upper part *at all*; and the effect on this upper point will be only that which arises from the excess of the tide at the lower point above the amount which is just perceptible above.

Hence the tide at the upper point will not rise during a time corresponding to the whole rise below, but only during a certain portion of the time of highest water; and if we call the state of the river before it begins to rise, *low water*, the time from low water to high water will be less than six hours, and may be much less. This interval also, as follows from the above reasoning, will diminish in ascending the river.

Mr. RENNIE's observations give us the means of verifying the statement just made. It appears that the interval from low water to high water thus taken, is about four hours at Putney, three hours at Kew Bridge, two hours at Richmond Bridge, and one hour and three quarters at Teddington.

In channels open at both ends, the time of high water and the time of slack water are generally different, as has already been explained; but in a river, if the tide-current ever do overcome the river stream, so as to make the water run up, the time of highest water will be very near the time of slack water. At Richmond these times are not found to differ more than a few minutes. It is to be observed, however, that there may be tides in a river without any up-current. The diminution of the velocity of any part will make the water accumulate behind that part; and we may have the tides rise to any height, though the water is flowing down all the while. We have already described such a case.

In open seas, the same kind of curve forms the surface at all periods of the tide, and its convex-parts are carried from one place to another. But in rivers, the form and condition of the surface are quite different at high and at low water; and the tide elevation, which is carried from one place to another, has at every place a different relation to the average surface.

We may consider the tides in the Thames, for instance, to take place in the following manner.

When not affected by the tide, the surface of the water slopes so that, compared with Trinity high water mark, it is below it by the following quantities:

	ft.	in.
At Teddington	1	2
— Richmond	4	9
— Kew	8	6
— Putney	12	0
— Old Swan	15	3
Fall at London Bridge (the old bridge).		
— Billingsgate	19	7

But when it is high water, the whole surface becomes much more nearly level; for we have, taking the mean of the same two groups of four days, which we used before;

Declivity at high water,

	ft.	in.	ft.	in.
From Teddington to Richmond Bridge	1	5⅜	2	0¾
—— Richmond Bridge to Kew Bridge	0	5⅝	0	9¼
—— Kew Bridge to Putney Bridge	0	7	0	9½

These high waters are not exactly contemporaneous; but this circumstance will not affect the general facts. It appears therefore that the tide raises the surface of the lower part of the river, so as to diminish the slope of that part; that this altered surface extends successively higher and higher up the river, and is nearly horizontal at high water; after that, the tide-surface again descends, and the effects of it disappear in succession from the parts of the river, beginning with the upper parts, till the low water surface is again attained.

Velocity of the Tide-wave in Rivers.

The velocity of the tide-wave in these as in other cases must be estimated by the rate at which the points of high water are transferred.

Tides of the River Thames.

Taking the mean of the same days as before, I find time of high water travelling

From London Bridge* to Putney (distance, 7⅜ miles) . .	$0^{h}\,31^{m}$.	36^{m}
—— Putney to Kew (distance, 5¾ miles)	0 17½ .	23¾
—— Kew to Richmond (distance, 3 miles)	0 22½ .	26¼
—— Richmond to Teddington (distance, 2¾ miles) . . .	1 18¾ .	37½

It is clearly seen here that the velocity of the wave diminishes as we ascend the river.

The whole distance from London Bridge to Richmond is sixteen miles, and it appears that the mean of the whole times employed is $1^{h}\ 18^{m}$, which gives an average velocity of about twelve miles an hour.

It will follow from what has already been said, that the point of low water travels up the river with a velocity different from the point of high water. Taking a group of four days, I have the following mean time of *low water* travelling

From London Bridge to Putney.	
—— Putney to Kew	1^{h} $3\frac{3}{4}^{m}$
—— Kew to Richmond	1 30
—— Richmond to Teddington . . .	1 30

I add the following statements of the motion of the tide-wave in rivers.

Tides of the Bristol Channel and its Rivers.

The tides of the Bristol Channel may be considered as river tides. The following is the progress of the tide-wave, as determined principally by the survey of Lieut. DENHAM.

	H. W. Time.	Height of High Tides.
Lundy Island	5^{h} 15^{m} . . .	27 feet.
Hartland	5 20 . . .	26
Appledore	5 30	
Barnstaple Reach (6 miles up) .	6 0	
Bideford Branch	6 0	

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* The time at London Bridge is here found by adding 10^{m} to the observed time at the London Docks.

	H. W. Time.		Height of High Tides.
Ilfracombe	5^h 45^m		
Fairland	6 10		33 feet.
Minehead	6 30		38
Sully	6 45*		31
King's Road	6 45	(Norie)	46
Bristol	7 0	——	40
Chepstow	7 30	——	70

From Hartland Point to King's Road is about ninety miles; it appears that the tide travels over this distance in about an hour and a half. The tide increases as it advances.

Tides of the Frith of Forth and its River. (Stevenson's Bell Rock Lighthouse, p. 80.)

Time of high water at

Bell Rock	1^h 30^m
Can Rock	1 45
Elie	2 0
Kinghornness	2 15
Queensferry	2 45
Alloa	3 45

Distance of Alloa from the Bell Rock, about seventy miles.

Tides of the River Waveney (Suffolk).

This river enters the sea at Yarmouth, and communicates by small and tortuous channels with the Lake Lothing near Lowestoff, which lake is separated from the sea only by a narrow strip of sand-hills. The distance of this communication is not above fifteen miles; but the channels were so small, that originally the tide occupied about six hours in reaching the lake; consequently, it was high water in the lake when it was low water in the sea on the other side of the bank, and *vice versâ*.

The canal lately made in the formation of the Lowestoff navigation has substituted a shorter and straighter course for the former communication, and the time of high water at the head of the channels is rendered earlier by an hour or two. Also, a communication has now been cut between the lake and the sea, and flood-gates placed so as to open either way, as the state of the tide may require.

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* From a fortnight's observations of the Rev. W. D. Conybeare:—communicated by him.

Tides of the River Gironde. (Daussy.)

Times of high water:

	H. W. Time.
Cordovan	$3^h\ 59^m$
Saint-Surin	4 33
La Marechale	5 0
Patiras (Isle)	5 21
Blaye	5 35
Bordeaux	6 54

Distance from Cordovan to Bordeaux, about fifty miles.

Tides of the River Elbe.

	H. W. Time.
Heligoland	$11^h\ 0^m$
Red Buoy	12 0
Cuxhaven	1 0
Hamburgh	6 0 (Norie.)

Distance, from Cuxhaven to Hamburgh, about sixty miles.

Tides of the River Weser.

	H. W. Time.
Island of Wrangeroog	$12^h\ 0^m$
Bremen	6 0 (Norie.)

Distance, sixty miles.

Tides of the Gulf and River of St. Lawrence.

The tides of the Gulf and River of St. Lawrence have been observed in several places in the course of the survey of those coasts executed by Captain Bayfield and Lieutenant Collins.

The tide-wave enters the Gulf between Cape Ray (the south point of Newfoundland,) and Cape Breton, where the tide-hour (Greenwich time) is about 1^h. It proceeds west to the Isle of Anticosti, at the east end of which there is a point of divergence, and a point of convergence at the west end. We have the following data, in the time of the respective places:

	Long. E. of Quebec.	H. W. Time.	Height.
Mingan (N. shore)	8° 0′	2^h 20^m	9 feet.
Mahane	3 40	2 0	
Bic	2 21	2 15	14
Bersemis	2 30	2 15	
Goreen Island	2 50	2 15	16
Tadousac		3 0	
Hare Island		3 15	
Kamourasca	2 20	3 45	
Coudres		4 40	17
St. Thomas		5 15	18
Isle of Orleans		5 45	18
Quebec		6 45	20

The longitude of Quebec is 71° 10′, or in time 4^h 45^m. Therefore the tide-hour, Greenwich time, at Mahane is 6^h 30^m nearly, and at Quebec 11^h 30^m.

We may observe here, as in the Bristol Channel and in the Garonne, the increase of height in the tide as it advances.

The distance from Mahane to Quebec along the river is about three hundred and fifty miles. The tide-wave occupies, as we see, about 5 hours in travelling over this space.

Sect. IV. *General Remarks on the Course of the Tides.*

1. *On the Velocity of the Tide-wave.*

The ridge of water which brings the tide moves from the position of one of the horary cotidal lines to that of the next, in an hour; hence the velocity with which this wave advances will be measured by the distance of two such lines, this distance being taken in a direction nearly perpendicular to both. It will be seen by a glance at the map, that the velocity so measured is very different in different places. In latitude 60° S., where the sea is not interrupted by any land, except the narrow promontory of Patagonia, nearly the whole circumference of the globe must be occupied by twenty-four cotidal lines; hence the velocity in an hour will be about 670 miles. In the Atlantic Ocean, the distances of the lines appear to be in some cases 10° of latitude, or near 700 miles. This wave travels from the south point of Ireland to the north point of Scotland in about eight hours; this distance is about 7°, or 160 miles an hour along the shore. On the eastern coast the velocity is less; from Buchanness to Sunderland, three hours, gives about sixty miles an hour; from Scarborough to Cromer, also three hours, about thirty-five miles; from the North Foreland to London in two hours, makes the speed about thirty miles an hour; from London to Richmond in one hour and a quarter, gives us thirteen miles in that part of the river.

The velocity which results from theoretical considerations is not easily determined. **Newton**'s solution of the problem of waves proceeds upon a supposition of the motion of the fluid so different from the real case, that it has no claim to be considered even an approximation. If we suppose any accumulation of fluid in one part of an ocean, this accumulation would transmit its pressure in all directions; and by the play of such pressures in different parts of the fluid, motions of the parts of the fluid would be produced, which would transfer the elevation of the surface to other situations, as really takes place. But it is very difficult to deduce the results of this action, without additional suppositions. One of the simplest suppositions is that on which **Lagrange** gave the solution; namely, that the horizontal motion of the parts of the fluid is the same all the way to the bottom. On this hypothesis, the velocity with which a wave is transmitted along a uniform channel, is that which a heavy body would acquire by falling freely through half the depth of the fluid. On other suppositions, the velocity of transmission would depend on the dimensions of the wave.

The velocity of waves has not been very completely determined by experiment. In **Weber**'s experiments (Wellenlehre, p. 172), the velocity in a channel six inches deep was nearly that which **Lagrange**'s theory would give, but the velocity did not increase so fast by increasing the depth, as the theory would require. Perhaps we might represent the results by supposing that in deeper channels a portion of the *depth* is not *effective*, the water at the bottom not being affected by the horizontal motion.

Nor have we any good observations of the velocity of waves at sea. A persuasion appears to obtain that the velocity depends on the breadth of the wave, and that the larger waves travel fastest.

The tide-*wave* would be transmitted by the same laws of the effects of pressure in fluids, which produce the transmission of other waves, though the tide-wave is so different from others in its dimensions, being very broad and flat. On the east coast of England, for instance, the breadth of this wave reaches from the Orkneys to the coast of Norfolk, which are the places where its two ridges contemporaneously are,—the hollow of the wave being then on the coast of Yorkshire, and perhaps twelve or fourteen feet lower than the ridges. And in the Atlantic the wave is still wider, one ridge being in the Antarctic Ocean, while the other is at Newfoundland.

It will be seen from what has been said, that we cannot trace very distinctly the causes which produce so great differences in the velocity of the tide-wave in different places. The difference of depth has probably a great share in producing these differences; and accordingly we find that in narrow seas, which are also generally the shallowest parts of the ocean, the velocity is much diminished, and the tide-waves consequently *thronged:* and this fact appears with remarkable uniformity in every part of the map. Other causes of the difference of velocity are probably the obstacles to motion offered by the uneven form of the bottom and shores: and as we are in a great measure ignorant of the form of the bottom of the sea, we may easily conceive that the course of the cotidal lines may exhibit many inexplicable anomalies.

If we calculate the *effective depth* of different parts of the fluid on LAGRANGE's principle, from the velocities above stated, we find that the effective depth of the Thames from London to Richmond is 13 feet; from the Nore to London, 90 feet: the effective depth of the sea on the east coast of England, 120 feet; of Scotland, 360; of the Atlantic, west of Ireland, 2600 feet; of the Atlantic in its middle parts, 50,000 feet, or above nine miles.

2. *On the Form of the Cotidal Lines.*

It will be seen in the map that the form of the cotidal lines is very various, and apparently anomalous. Still they conform in most of their general circumstances to the principles which were stated at the outset. They are convex in the direction of their motion, the ends near the shore being held back by the smaller velocity in shallower water and other resistances, agreeably to what has just been stated: they bend round promontories and islands, forming a point of divergence on the side of the coming tide; tides which meet two distant points of the same shore at the same or nearly the same time, produce a point of convergence between them; and other features appear, such as the theory requires.

It may, perhaps, appear to some persons that the forms of the cotidal lines, as I have given them, are in some cases too irregular and complex to be probable; as for instance, on the west coast of America. To this I can only say, that they are the simplest forms, and the most agreeable to the general rules of the rest, by which I can connect such observations as we possess. If it shall appear that our observations are inaccurate, the lines may very probably be in reality simpler than they are here represented: and if this representation be at all correct, it ought to be confirmed by observations made at many adjacent points on the coasts which exhibit these complex forms.

There is one inaccuracy in the position of these lines which I have not attempted to remove, as it does not materially alter their shape. The times of high water from which the lines are constructed, are the times *at* the full and change of the moon; but the times for which they ought to be constructed, are the times of the tides *corresponding to* the full and change, which tides may be one, two, or more days after the syzygy. Thus, the time which we have used for Plymouth is $5^h\ 33^m$, that being the time of high water at full and change, or the *vulgar* establishment. But the high water at Plymouth corresponding to the moon's syzygy is two days and a half after the change, at which time the tide is only $5^h\ 3^m$ after the moon, this being the *corrected* establishment. This cause, however, will not alter the form of the line; for it applies equally to the tide at every point of the same line: the only consequence is, that the line which is taken to belong to $5^h\ 33^m$, does in fact belong to $5^h\ 3^m$, *corrected* establishment; and the $5^h\ 33^m$ line, similarly corrected, would be a little in advance of the position we have assigned; and each of the other lines will in like manner require to be thrown forwards by a small quantity proportional to the age of the tide. When the course of the lines is obtained with tolerable accuracy, this can easily be done.

3. *On Tide-Currents.*

The motions of the currents produced by the tides are, in most circumstances, much more important and interesting to the sailor than the motion of the cotidal lines: but their laws are far more complex and difficult than the laws of the transmission of high water, nor shall I at present attempt to treat of them in detail. I will observe, however, that complex as the problem is, I do not think it would be a desperate undertaking to assign some of the leading rules of the tide-currents, if the rules of high water were accurately and extensively ascertained; a good approximation to the solution of the problem on which we are now engaged must precede anything like a scientific view of the phenomena of tide-currents. I will only offer a few remarks.

One of the most important circumstances of this subject is that which has been already stated,—that in an open channel the *flood current* (the current which runs till high water) will continue running for three hours afterwards, or till *half-ebb*; and the ebb current, which then begins, will run after low water till half-flood. The time of slack water is intermediate between the times of high and low water.

In proportion as the channel is obstructed at the further end, the flood current runs for a shorter time after the flood; and in a closed creek the flood current ends at high water.

Thus in the British Channel the flood current runs three hours after high water; in the Bristol Channel, two hours after high water; in rivers, only a few minutes after.

Revolving Tide-Currents.

We have, in what precedes, spoken only of currents which run alternately in two opposite directions; but in many seas the currents at successive periods shift into many new directions. In particular, they in many places revolve in twelve hours through the whole circuit of the compass. This is noticed with regard to the tide-currents among the Scilly Islands by SPENCE (p. 4.), and has been particularly attended to by Captain M. WHITE and other officers who have surveyed our shores. The Dutch have noticed this phenomenon on their own coasts; and have expressed it on their charts by a peculiar mode of representation. It has been expressed in a different manner by Captain THOMAS in a

Chart of the German Ocean: we may hereafter say a few words on the selection of a mode of denoting the facts of such cases. As a specimen of these cases I give the following from Captain WHITE:

Stream of tide off Scilly in the vicinity of the Poul Bank:

From low water to half-flood . . .	N. 45° W. velocity	1 mile per hour.
—— half-flood to 4 hours flood . .	N. 22 E. . . .	1 ———
—— 4 hours flood to high water . .	N. 56 E. . . .	1·6 ———
—— high water to quarter-ebb . .	S. 67 E. . . .	·75 ———
—— quarter-ebb to half-ebb . . .	S. 22 E. . . .	·75 ———
—— half-ebb to three quarters ebb .	S. 22 W. . .	·5 ———
—— three quarters ebb to low water .	W.	1·0 ———

It will be seen from this, that here the current goes round from the west by the north to the east, and so to the south; that is, it revolves in the same direction as the sun does.

These revolving currents take place especially in the neighbourhood of places where the streams of tide separate or meet: and it is not difficult to see that these are the situations in which they might be expected to occur. Thus at the Scilly Islands, the Atlantic tide-stream divides into two; one of which runs up the Bristol and St. George's Channels, while the other proceeds along the English Channel. At the point where the stream bifurcates, if the two branch currents, while they are in contact, exactly balance each other in lateral pressure, they may run in constant directions; but considering the various circumstances which influence each of the two, this can hardly be the case for the whole time of their motion; if one of the currents which are thus in contact become more powerful than the other, it overrides and turns it, and thus gives rise to a change of direction; and as the circumstances of each current go through a regular cycle in twelve hours, it may easily be imagined that the direction of the stream may go round the compass in that time.

It would not be to our present purpose to trace much further the details of such cases, but we may observe that they have attracted notice as supposed indications of the direction in which the tide comes. Thus it has been observed, that on the Dutch coast the tides northward of the Oude Steen bank of Goeree follow the course of the sun, while at the entrance of the Schelde and off Flushing they revolve in the opposite direction. (Sailing Directions, North Sea, p. 54.)

We may observe also, that we have frequently near the shore a *counter stream* running in a direction opposite to the main tide stream which prevails further seawards, (for example, the north-east coast of Ireland). This commonly occurs where the stream is impeded in its progress by some projecting shore or island; and the opposite currents which are thus brought in contact often produce remarkable effects, especially where the promontories are of very sudden projection.

Where a promontory does not produce a counter current, the stream which advances along the shore is thrown obliquely into the main stream by the bend of the land, and we have thus various phenomena, such as the Race of Portland and other similar cases.

4. *On the Magnitude of Tides.*

The magnitude of tides is very various, and the causes of the differences are often far from obvious. I shall notice some of them.

I shall not endeavour to determine the magnitude of the tide according to theory. The smallness of the tide in the middle of wide oceans, as in the islands of the Pacific, where it amounts only to two or three feet, is not to be taken as a proof that this would be the amount on a globe entirely covered with water; for the interruption produced by the continents entirely modifies the circumstances of the general tide. This interruption will diminish the original tide; and the derivative tide which enters such oceans from the south-east, is *diffused* over so wide a space that its amount is also greatly reduced.

In the same manner in which the tides are diminished by diffusion, they are augmented by convergence. The greatest shore tides occur near points of convergence; and when there is added to this a contracting bay or river channel, we have tides of extraordinary height. Thus the head of the Bristol Channel, of the Bay of St. Malo, of the Bay of Fundy, have very large tides.

There is one difficulty which it may be well to notice. Since the tides in places near each other are of different heights, the surface of the water cannot be level: "This unequal rise," says Mr. SPENCE, "is in a manner directly contrary to the known law of all fluids, by which they run to the same level at all neighbouring places, and rise to equal heights from the centre of the earth; as, for instance, at Cape Clear, springs rise only eleven feet perpendicular, but at Scilly twenty feet. At Tusker and Wicklow, springs rise only four or five feet, but in the vicinity of St. David's Head thirteen feet. At Brest I understand that springs rise only three or four feet*, but at Falmouth sixteen or seventeen feet. Among the Norman Isles four or five fathoms is not an uncommon rise, but at the very extremity of Portland Bill I have taken a favourable opportunity to observe the greatest rise of a spring, and found it scarce seven feet."

The difficulty thus stated is not without weight; the solution of it appears to be this;—that the water, on a great scale, cannot be considered as at rest, and that therefore the form of the surface is not that of equilibrium. We may perceive that in a stream of water in rapid motion, as in a mill race, there is a *permanent* curvature of a part of the surface, showing that a fluid in which the parts are moving with different velocities does not assume a surface exactly horizontal. There is a certain form of the surface, such, that with it the forces arising from the weight and the velocity of the parts of the fluid balance each other hydrostatically; and this is a surface such as admits of these inequalities of rise of the tide at different places.

So long as the velocities of the different parts retain their amount, the surface is just as necessarily determined, as it would be determined to be horizontal if all the parts were at rest. Hence if, under such circumstances, the level of one part be altered, the level of all the other parts will be altered in a corresponding manner. This will explain why the *times* of high water at neighbouring places are so nearly the same, though the heights are very different. On the coasts of England and France, for example, if the water fall on one side, this depression is transmitted through the fluid to the other side in a very short time; and in nearly the same time, whether the fluid, independently of this transmission, was at rest or in motion. The supposition of its being already in motion allows of a different level on the two sides; and as the motion may be different at different times, the side which is highest at one time may be lowest at another. Thus, combining the hydrostatical effect of the currents with the laws of transmitted undulations, we perceive how the form and motion of the cotidal lines may be very regular, while the tides along the same cotidal line are very different in their magnitudes, and form a very irregular series.

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* This is an error.

In all the cases which have been carefully examined, the time of high water at neighbouring places has been found to vary continuously and slowly in advancing along the sea; it is probable therefore that the statements of tides at very different hours at adjacent places will, in most instances, be found incorrect.

We may observe, however, that the relative level of the water at neighbouring places may change, by a change in the contiguous currents, and this change may be combined with the transmission of the undulation. The relation of contiguous currents may be different at springs and neaps, in consequence of the different supply of water; and from this cause, and from the difference of depth, the interval of times of high water at neighbouring places may be different at springs and at neaps. If this were the case, it would appear, in the observations, by the times of high water, in the course of a semilunation, having different relations to the establishment. If we make a curve, of which the ordinates represent the interval of time by which the tide follows the moon, this curve would be of different form at two such places.

5. *On the Constancy of the Cotidal Lines.*

A doubt may very naturally occur to the reader, whether the cotidal lines which I have endeavoured to trace, have a permanent position on the globe, or whether they do not vary from one tide to another, according to the age of the moon, or to other causes of change. They are here drawn for the tides which occur at new and full moon;—would their form be the same at any other period of the lunation? and may they not be affected by accidental causes, as winds and other atmospheric influences? May they not also experience secular changes, and vary from century to century? To these questions we have no means at present of returning answers with any certainty and accuracy. Our knowledge is yet very imperfect and very doubtful, with regard to the data on which the first approximation to the position of the lines must be conducted. When this is obtained, very multiplied and accurate observations may enable us to determine their monthly and other variations, if such exist.

The mode in which such variations will manifest themselves, will be this;—the *laws* of the time of high water will be different at different points. The curve of which we have just spoken, of which the ordinates are the intervals by which the tide follows the moon, will be different for different places of observation.

It appears probable that the monthly variation is not considerable; for, so far as observations have been made, the times of high water at most places appear to follow nearly the same laws. In the case examined by Mr. LUBBOCK, (page 19 of the present volume,) this resemblance is very striking. Mr. DESSIOU has there constructed the curves of which I have spoken, for Brest, Plymouth, Portsmouth, Sheerness, and London, and their identity is almost complete. But it would be interesting to have observations of the same kind made in distant seas, where the tide arrives by roads altogether different, instead of comparing branches of the same derivative tide, as all these are.

It appears likely, also, that there will be *some* variation owing to this cause; for the tide of many places may be considered as composed of parts which come to the place by different roads, and the relation of these parts will be affected by the changes of a lunation. If we subtract four feet from the height of each of two partial tides which come in different directions, their proportion will be altered, and thus the time of the component tide may be altered also.

As to the atmospheric causes of variation, we may say in like manner, that the partial irregularities of the tides show that there are some such variations, while the general steadiness of the laws of tide phenomena proves that these variations are not considerable.

With respect to the existence of secular changes in the phenomena of the tides, we are still more in the dark. The formation and removal of sand-banks and bars, the silting-up of rivers, and similar causes, may, and on a small scale must, produce such effects. Mr. LUBBOCK has pointed out (Philosophical Transactions, 1831, p. 389,) the probability that high water in the Port of London takes place at present earlier than it did formerly. The tide observations directed by Mr. RENNIE at Putney, Kew, Richmond, and Teddington, will, it may be hoped, discover the changes produced in the tides of the upper part of the Thames by the removal of Old London Bridge. Whether such changes take place on a larger scale it is impossible at present to say. It is conceivable that the alteration in the depth and form of the bottom of the ocean produced by great marine currents, and by the materials they bring or take away, may affect the velocity, and, by means of this, the form of the cotidal lines which cross the ocean. The change must be very slow, if it take place at all.

6. *On some Peculiarities of the Tides.*

Difference of the two diurnal Tides.—It has been remarked in various places, by separate observers, that the evening tide is higher than the morning tide at one part of the year, and lower at another. I extract below a few notices to this effect*.

These detached statements are confirmed by a more exact examination. The Plymouth observations show the difference, though to a small amount. **Laplace**, from his examination of the earlier series of observations at Brest, found about half a foot for the excess of the evening tide over the morning tide at the summer solstice. The examination of the later observations at Brest gave the result slightly larger. (Supp. Mec. Cél. liv. xiii. p. 161.)

This peculiarity was explained by **Newton**. From the vernal to the autumnal equinox the sun has north declination; and as the moon's orbit is never much inclined to the sun's, a line drawn from the earth's centre to the moon would meet the earth's surface, on the side towards the sun, in north latitude. Now such a line is the axis of the tide spheroid, supposing the tide to be always under the moon; and the tide taking place when the moon is in the meridian, is higher as the place is nearer to the vertices or points where this axis of the tide spheroid meets the earth's surface. Hence, in this case, the tides which occur on the side of the earth next the sun, or the day tides, would be larger for a place in north latitude than the tides on the opposite side. For a similar reason the night tides would be highest in winter*.

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* Philosophical Transactions, vol. iii. p. 814. Captain **Sturmy**, at Hong-road near Bristol, says, "Concerning our diurnal tides we observe, that from about the latter end of March till the latter end of September, they are about 1 ft. 3 in. higher perpendicularly in the evening than in the morning; that is, if high water happen after the sun is past the meridian, or in the tides betwixt noon and midnight: but from Michaelmas till our Lady Day we find the contrary, the day-tides being in that higher by 15 in. than the night tides, or the tides between midnight and noon."

Vol. iii. p. 633. **Colepress**, at Plymouth, says, "Our diurnal tides from about the latter end of March till the latter end of September, are about a foot higher in the evening than in the morning, that is, every tide which happens after 12 in the day before 12 at night; and *vice versâ* the rest of the year.

Asiatic Researches, 1829, P. 1, p. 262. **Kyd** on the Tides of the Hoogly: "There is another local affection of the tides, the cause of which I cannot satisfactorily explain. In the north-east monsoon [from the end of October to the beginning of March] the night tides are the highest, whilst in the south-west monsoon [from March to October] the day tides are highest."

This would be so on the supposition that the tide took place when the moon was on the meridian; but if the tide take place 6 hours after the moon's transit, the pole of the spheroid having still the same declination as the moon, the pole which follows the moon will be north of the equator, from the time when the moon is 6 hours to the west of the sun till the time when she is 6 hours to the east of him; that is, from the time when the tide is at noon, to the time when it is at midnight: after this the moon goes south of the equator, the largest tide is that opposite to her, and this again happens at times which advance successively from noon to midnight.

But if the tide take place 18 hours after the corresponding transit of the moon, it will appear by similar reasoning, that the lower tide will take place from noon to midnight when the sun is north of the equator, and vice versâ.

If the tide be at 30, or at 54 hours after the transit, the phenomena will be the same as when it is at 6 hours after.

Now the last case appears to be that of the phenomena at Brest, Plymouth†, and Bristol; therefore the tide of these places cannot be 18 hours old, and

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* Laplace has objected to this explanation,—that if it were true, the two semi-diurnal tides at Brest, when the moon has her greatest declination, ought to differ in the ratio of 8 to 1, whereas their difference is very small. But it is clear that Newton's theory must be applied, as Laplace's also requires to be applied, by considering the tides on our coasts as derivative tides from those in the Southern Ocean. When the moon has considerable south declination, it is clear that the superior tide in the Southern Ocean will exceed the inferior tide; *how much* it will exceed it, must depend on the form of the ocean. Laplace is not able by his method, any more than Newton by his, to calculate the excess *à priori*.

† There is an anomaly, as yet unexplained, in the Plymouth Tide-Observations. From the course of the cotidal lines it would appear that the Plymouth tides are only about 1 hour 35 minutes after those at Brest; yet it appears by the examination of the observations, that the age of the tide at Brest is about 1½ day, and at Plymouth 2½ days. The amount of the latter is as great as the age of the tide at London; which circumstance it is difficult to account for, as Mr. Lubbock has already noticed (see his "Note on the Tides").

It may be observed that the difference between the law of the Plymouth tides (as given by the obmust be either 6, or 30, or 54 hours; it is certainly more than the former, therefore its age is probably 30 hours or a little more, which agrees with what we know from other considerations.

In the port of London this difference of day and night tides is not perceived, for the tide there is compounded of two, distant by 12 hours from each other, and therefore of a larger and smaller tide in each case.

We appear at first sight to have an exception to the rule now stated, in the case of New Holland. It is asserted by Captains Cook, Flinders, and King, that the night tides there are *always* greater than the day tides. Cook was there in *August* 1770, Flinders in *January* 1802, and Captain King in *January* 1822, and therefore the observation has been made both when the sun was north and when the sun was south of the equator, which appears to contradict the rule of the other cases.

But on examining further, we find that the observations took place on different parts of the coast. Cook's were made on the eastern side, in consequence of the Endeavour getting within the reef: here the tide-hour is 8; and as the high tide occurs 8 hours after the moon's transit *on the other side* of the equator, the age of the tide here must be about 20 hours. Captain Flinders made his observations at King George's Sound and the neighbouring parts, where the tide is 7 or 8 hours later than on the western coast, which throws it into the next half-day, and reverses the phenomena of morning and evening. Captain King confirmed Captain Flinders's observations on this part of the coast.

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servations,) and those of other places is easily shown, without any peculiar mode of considering them. The greatest and least intervals between the moon's transit and the tide are as follows, (see Mr. Lubbock's Paper):

	Moon's Transit.	Greatest Interval.	Moon's Transit.	Least Interval.	Difference of Intervals.
	h m	h m	h m	h m	h m
Brest	9 30	4 8	5 0	2 49	1 19
Plymouth........	10 0	5 48	6 0	4 12	1 36
Portsmouth	9 30	12 2	5 0	10 41	1 21
Sheerness........	10 0	12 54	6 0	11 25	1 29
London...........	10 15	2 10	6 0	12 42	1 28

The difference of the greatest and least intervals ought to be the same in *simple* tides at all places. The above variety of values of this interval *may* be owing to the different manner in which the tides at the different places are compounded.

Probably a good series of tide-observations at the Eddystone would throw light upon this apparent anomaly. It would be easy to suggest the means by which they might be made.

Single Day Tides.—In some places there is high water once only in twenty-four hours. The most celebrated of such cases is that of the harbour of Tonquin, described by Mr. DAVENPORT, attempted to be reduced to rule by HALLEY, and explained from theory by NEWTON. The circumstances of that case are as follow:

The tide rises and falls every day during about 12 hours each way. The rise begins every successive day later by about three quarters of an hour, so that in 15 days the time of high water advances from 1 o'clock in the afternoon, for instance, to 12 at midnight; after which it does not advance to 1 in the morning, but falls back 13 hours to 12 o'clock at noon, and so on perpetually. In this way the high water is always in the afternoon during the summer half-year (March to October), and in the forenoon during the remaining half. (Philosophical Transactions, vol. xiv. p. 162.) About the time when the tide time falls back 13 hours, the tides are very small and scarcely perceptible; at the intermediate times they are greatest.

NEWTON'S explanation of these phenomena consisted in supposing that the tides at this place are compounded of two tides arriving by different paths, one 6 hours longer than the other. When the moon is in the equator, the morning and evening tides of each component tide are equal, and the tides obliterate each other by interference, which takes place about the equinoxes. At other periods the higher tides of each component daily pair are compounded into a tide which takes place at the intermediate time, that is, once a day; and this time will be after noon or before, according to the time of year, as will appear by the reasoning of last section*.

This explanation is extremely probable, and requires only to be confirmed by observations of the tides of the adjacent parts, which might give the course of the two component tides before they meet.

It has already been mentioned, that at Juan Fernandez the tide is said to run 12 hours each way; and the same is stated to occur in other cases. But the only instances in which we have accurate accounts of such phenomena are two others, which also are found in the Indian Ocean, one on the north and one on the south coast of New Holland. I shall quote the descriptions of these from Captains Flinders and King[*].

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* I refer to NEWTON'S explanation rather than LAPLACE'S, as more familiar to the common reader. The two explanations appear to me to coincide, both in the forces to which they attribute the fact, and in the laws of the phenomena to which they lead.

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[*] *Tides at King George's Sound; south coast of New Holland,* long. $7^h\ 52^m$ E. Flinders, i. p. 71. January 1802.—"According to Lieut. Flinders's observations on shore during 16 days, there was only one high water in 24 hours, which always took place between 6 and 12 at night; for after, by gradually becoming later, it had been high water at 12, the next night it took place soon after 6 o'clock, and then happened later by three quarters of an hour each night, as before. The greatest rise was 3 feet 2 inches, and the least 2 feet 8 inches.

"The accumulation was made in this manner: after low water it rose for several hours, then ceased and became stationary, or perhaps fell back a little. In a few hours it began to rise again; and in about 12 from the first was high water.

"It was observed by Capt. Cook upon the east coast of this country[a], and since by many others, including myself, that the night tide rose considerably higher than that of the day, which is conformable to our observations in King George's Sound, but with this difference, that in the day we had scarcely any tide at all."

King, ii. p. 380, 381, January 1822. Oyster Harbour, a branch of King George's Sound.—"During the springs, high water always takes place at night. The flood tide in the entrance generally ran 16 hours, and ebbed 8 hours. High water at full and change took place at $10^h\ 10^m$ at night; but on the bar the rise and fall was very irregular, and a vessel going in should pay great attention to the depth, if her draught is more than 10 feet; for it sometimes rises suddenly 2 feet. The spring tides take place about the third or fourth day after new or full moon."

Tides at Wellesley's Islands; north coast of New Holland, long. $9^h\ 19^m$ in the Gulf of Carpentaria. Flinders, ii. p. 149. November, 1802.—"The tides in the Investigator's Road [the channel between the islands and the main land,] ran N.N.E. and S.S.W. as the channel lies, and their greatest rate at the springs was 1¼ mile per hour; they ran with regularity, but there was only one flood and one ebb in the day."

ii. p. 155. Bountiful Island, near the former.—"From a little past 10 in the morning to 11 at night the tide ran ½ a mile an hour to the S.W., and N.E. during the remainder of the 24 hours; the first, which seemed to be the flood, was only 3 hours after the moon, above 6 hours earlier than in the Investigator's Road: but the time of high water by the shore might be very different; no greater rise than 5 feet was perceived by the lead line."

I add the following notices from Purdy (A. M. p. 76.), which seem to refer to tides of this kind in the West Indies.

Vera Cruz. Only 1 tide in 24 hours: height 2 feet.

About the Island of St. Bartholomew the flood at new and full moon runs S.E., and it is then high water at $10^h\ 30^m$ P.M. while the sun is furthest to the north of the equator; but comes about two hours sooner in the following months, till the sun gets furthest to the south, when it is high water at $10^h\ 30^m$ A.M., and it runs afterwards in the same proportion back again. The greatest difference in the ebbing and flowing is 18 inches, but in general only 10 inches.

The following is not obviously of this class. (Ibid.)

"In *Ponce, or Chatham Bay* (S. end of Florida), it runs 3 hours flood, then 3 hours ebb; next 9 hours flood," &c.

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[a] Hawkesworth's Voyages, vol. iii. p. 647.

There can be little doubt that these are cases of interfering tides, but observations at several adjacent places would be requisite to determine the paths by which the interfering tides arrive. In the case of Wellesley Islands in the Gulf of Carpentaria, the extremely broken form of the land north of New Holland allows us easily to suppose such different paths for the tide-wave. At King George's Sound on the south, this condition is less obvious. It is not impossible that the tides in this part may be affected by an undulation propagated from the Indian or South Atlantic Oceans by the way of the south pole.

Double Half-day Tides.—In some places we have more than two tides a day. Poole in Dorsetshire is an instance of this.

"*Poole Harbour* has an uncommon advantage, namely, that of the tide ebbing and flowing twice in 12 hours. It is low water at about half-past 3 o'clock, then flows regularly 5 hours and 20 minutes, and makes proper high water about 50 minutes after 8 o'clock. It then ebbs 1½ hour, and again flows 1½ hour, and then ebbs till low water.

"The second flood seems to be owing to the peculiar situation of the entrance; for by its being a bay towards the east, the tide of ebb from between the Isle of Wight and the main falls into that bay, and forces its way into the river, so as to raise the water for an hour and a half; at which period the water without the bar, by its falling below the level of that within, produces a second ebb for more than 3 hours, or till it is low water.

"In *Christchurch Harbour* the tides are nearly similar." (Dessiou, Sailing Directions, E. Channel, p. 83.)

This explanation is probably right in principle: the level is altered by the velocity of the ebb current near the shore; and this alteration of level, from the hydrostatical effect of currents, shows itself in the form of a second rise of the surface, after it has begun to descend from the true high water.

Weymouth Harbour is circumstanced nearly as Poole, and has also a double tide. "On the days of new and full moon it is high water on the shore in Weymouth Harbour about $6^h\ 30^m$; low water the first time about 11^h, and low water the second time about 2^h; so that with spring tides there is about 4½ hours between high water and the first low water, during which time the whole ebb falls; the tide then flows about 1½ hour, and rises a few inches, and again ebbs for about 2 hours and falls a few inches, but is not so low as the first low water. Neap tides sometimes remain stationary about low water for 2, 3, and sometimes 4 hours. From which it appears that there is only one high water with springs, but two low waters; and with neaps there are not two low waters observable, but a stationary tide.

"The tides in West Lulworth Cove are much the same as at Weymouth."—Mackenzie's Survey of the Channel (Admiralty MSS.).

Sect. V. *Suggestions for future Tide-Observations.*

It has appeared in the course of the preceding discussion, how extremely imperfect, and in many cases contradictory, are the statements which we at present possess concerning the establishments or tide-hours at different places. This has arisen in a great measure from the circumstance, that these observations were made without any settled rule or any definite object. If, with the opportunities which now exist, observations are for the future made with due attention to the circumstances of real importance, we may in a very few years be able to draw a map of cotidal lines with certainty and accuracy; and thus to give, upon a single sheet, a tide table for all parts of the earth.

1. *Of the Observation of the Height and Time of Tides.*

It is perhaps not necessary to go into any detail on the subject of the arrangements which may be useful in observing the height and time of each tide. Any person who gives his attention to the subject, and especially any naval person, will probably hit upon contrivances better adapted to the circumstances of his particular case, than could be pointed out by any general suggestions. It may, however, be not superfluous to mention one or two resources for the consideration of observers.

The instant to be observed is that when the surface of the water is highest. If the water be perfectly still, the surface changes very slowly when near the highest point, and appears to be stationary for some moments. To avoid the difficulty produced by this circumstance, some observers have observed, not the time when the water is highest, but two instants of equal height before and after the greatest; and the time of greatest height is supposed to bisect this interval.

In comparative observations, if the time observed in one case be that at which the surface begins to fall, it ought to be so in all cases; and similarly, if the time observed be that at which the surface ceases to rise.

To obviate the effect of waves in rendering the surface uncertain, the following apparatus may be used. Let a spout or pipe be fixed upright, in such a situation that at tide time the water reaches its lower part. The bottom of the pipe must be stopped, and a number of small holes (for instance, each half an inch in diameter) must be made in or near the bottom. A float nearly filling the pipe is to be placed in it, and to carry a light upright rod, divided into feet and inches, which are to be read off by means of an index or mark fastened to the top of the tube. The apertures in the bottom of the tube will allow the float to rise and fall with the general surface, without any sensible loss of time; while the smallness of these apertures will prevent the oscillations of the waves from affecting the inside of the tube. The moment when the rod, and consequently the surface, is highest, may be observed by means of the index.

The spout might be further protected, by having its lower end in an open box. It might be fixed to a post driven into the ground. If the most convenient place for it were at a distance from the observer, it might be observed by means of a telescope. If it were desirable, it would not be difficult to devise means by which the rod should pull a trigger and ring a bell the moment it began to descend; and thus mark the moment of high water without the trouble of watching.

Mr. MITCHELL's tide-gauge, which is in operation at Sheerness, marks the time of high water by means of a curve, of which the abscissas represent times, and the ordinates the corresponding heights. The greatest height during each tide is picked out by the eye, from the curve drawn by the instrument, and the corresponding time taken from the scale. This instrument is found to answer extremely well in practice, but could not be used except in stationary observations.

The time used in tide-observations may be mean or apparent time, but it should always be noticed which is employed, and by what means obtained.

2. *On finding the Establishment of any Place by Observation.*

The vulgar establishment of any place, or the time of high water at the full and change of the moon, may be determined roughly by an observation of the tide on the day of the full or change. We must, however, observe the following corrections.

1. The establishment should be expressed by saying, that the tide is, on the day of full and change, so many hours *after the moon's transit*,—not that it is *at* such an hour *of the day*. The mode of stating the result here recommended has been recently commonly employed by some of our best naval surveyors; for instance, Captain KING. The time of the moon's transit is of course easily known from the tables.

2. If the tide be observed according to mean time, and the time of the moon's transit be determined according to apparent time, it will be necessary to apply the equation of time to the interval.

3. The establishment of any place may be determined by observations not made at the full or change, by applying to the observed interval of the tide and moon's transit, a correction depending on the moon's distance from the sun.

This correction is by some authors made to depend on the *day of the moon's age*: but as this is a very inaccurate mode of determining the moon's distance from the sun, the correct method is to make the correction depend on the difference of right ascension of the sun and moon; that is, on the *time of the moon's transit* expressed in *apparent* time.

4. It is found, however, that the correction does not depend on the difference of right ascensions of the sun and moon on the day of the observation, but at a certain antecedent time. This time is antecedent by a different interval in different parts of the world, and we cannot as yet ascertain it for many places. The rule given in the Annuaire du Bureau des Longitudes supposes the interval to be 36 hours, which is probably nearly true for the western coasts of Europe and the eastern coasts of America, but is not generally true.

5. The correction to be applied may be determined by means of the following table, which is calculated for the moon's mean parallax, and would require slight modifications for variations of parallax.

Time of moon's transit at antecedent time	0	1	2	3	4	5	6	7	8	9	10	11	12 hours.
Correction	0	−16	−31	−41	−44	−31	0	+31	+44	+41	+31	+16	0 minutes.

The second line of numbers expresses the Semimenstrual Inequality of the interval of moon's transit and tide compared with the interval *corresponding* to the syzygy; and the correction of the observed interval is the excess of the semimenstrual inequality on the day of observation above the semimenstrual inequality on the day of syzygy.

Thus at London the tide corresponds to the distance of the sun and moon 2½ days antecedent. Hence on the day of syzygy, the tide corresponds to a difference of right ascension of the sun and moon, amounting to 10 hours; for in 2½ days the moon's motion in right ascension with regard to the sun is 2 hours. Therefore the tide-hour, on the day of new and full moon, is affected by the half-monthly inequality to the amount of 31 minutes additive. Suppose that when the moon's transit happens at 5^h, it is observed that the tide is 44 minutes after the moon. The tide corresponds to a transit 2½ days sooner, when the difference of right ascension of the sun and moon was 3^h, and the half-monthly inequality for such a difference is 41 minutes subtractive. Therefore, in consequence of the half-monthly inequality, the tide interval on this day will be less than at the syzygy by $31 + 41$ minutes, and the tide interval on the day of syzygy would be $31 + 41 + 44$, or $1^h\ 56^m$, which is the vulgar establishment.

6. *The corrected establishment* is the establishment corrected for the half-monthly inequality; or the interval of the moon's transit and tide, not *on* the day of syzygy, but *corresponding to* the day of syzygy.

It may be determined by observing the intervals of the moon's transit and tide every day for a semilunation, and taking the mean of them. If several semilunations (a whole number, of course,) be observed, the result will be more accurate.

If we know by how much the transit of the moon to which the tide corresponds, is antecedent to the transit next preceding the tide, we may obtain the corrected establishment from an observation of any tide:—thus, in the above case, the tide being 44 minutes after the moon, and the half-monthly inequality 41 minutes, the corrected establishment is $1^h\ 25^m$.

It would simplify all our reasonings concerning tides, to employ in all cases the corrected instead of the vulgar establishment. The observation of the tides for a fortnight would give a first approximation to this; and observations continued for some months would give considerable accuracy.

3. *On the Effects of the Age of the Tide.*

The interval of time, by which the difference of right ascension of the sun and moon, to which the tide corresponds, is antecedent to the difference when the tide takes place, is, as has already been stated, what we have called the *age of the tide*, and has been named by Mr. LUBBOCK, following LAPLACE, the *retard.*

Its effect appears in two circumstances:—

First, In the difference of the corrected and vulgar establishment of any place. The interval of the moon's transit and tide at syzygy is greater than the mean interval for a fortnight; the excess is the increase of the half-monthly inequality during the age of the tide, reckoned from syzygy.

Second, The greatest and least tides do not happen on the days of new or full moon, but one, two, or three days afterwards.

As, however, we have attended principally to the times of high water, omitting for the present the discussion of the heights in detail, we shall consider only the effect of the age of the tide as it affects the times.

The exact determination of the age of the tide requires a considerable number of observations. When it is determined at one place for any derivative tide, we may expect that the same derivative tide at any other places will be deducible from the age so determined by considering the time which the tide-wave employs in passing over the interval. But when original tides may be supposed to come into play, it is not so obvious what will be the relation of the age of the tide at different places; for instance, at the Cape of Good Hope and at Van Diemen's Land. It would be very desirable to have continued observations at such places.

4. *On the Mode of reducing Tide-Observations.*

The best mode of obtaining from a considerable series of tide-observations, at the same place, the establishment and the age of the tide, (the elements which would enable us to construct tide tables for any place,) appears to be that employed by Mr. LUBBOCK and Mr. DESSIOU, in their examination of the tides of the Port of London, and of other places. It is the following:—

The times of high water are arranged according to the half-hour of the moon's transit on the day of the tide:—thus, all those tides which took place when the moon passed the meridian between $0^h\,0^m$ and $0^h\,30^m$ (apparent time,) are put in one class; all those tides when the moon passed between $0^h\,30^m$ and $1^h\,0^m$ in another class; and so on.

The mean of all the times of transit in each class is taken, and the mean of the intervals of transit and high water. We have thus a series of times of transit, with the corresponding intervals of transit and high water.

The interpolation for other times is most easily performed by means of a curve, drawn on paper ruled into small squares. The times of transit being laid down as abscissas, the intervals of transit and high water are erected as ordinates, and a curve is drawn, approximating, as far as a regular form will allow, to the points thus found. This curve gives the intervals for *any* times of transit, and a table may be constructed by means of it.

The interval corresponding to the time of transit $0^h\,0^m$ is the vulgar establishment: the mean interval is the corrected establishment.

The age of the moon when the interval is equal to the mean interval (the age being taken from the last syzygy,) is the age of the tide.

In this deduction, we neglect the inequalities produced by the variations of the declination and parallax of the moon and sun, which belong to a more advanced stage of the approximation, and have been treated of by other authors.

It may be observed that the difference between the greatest and least intervals of transit and tide is the same, for simple tides, at all places; it amounts, for the mean parallax and declination of the moon, to $1^h\,28^m$.

We may observe also that the regularity of the tides is rather increased than diminished by their having to travel along a confined channel; the inequalities being extinguished by the narrowness of the passage, (as Laplace observes with respect to Brest,) like the oscillations of the mercury in the marine barometer. Nor does it appear that the irregularity of the channel much disturbs this result. The age of the tide might therefore be determined by means of observations made in an inlet or tide river. The establishment of the coast would not be thus obtained, but might be determined from comparatively few observations made at the mouth of the inlet or river.

5. *On tracing the Motion of the Tide-Wave by Comparative Observations.*

The places of the cotidal lines, and consequently the motion of the tide-wave, have been determined by means of such statements as we have been able to find, of the establishments of different places; and if the establishments were more correctly and extensively known, we might draw these lines with more accuracy and completeness.

But it is often possible to trace the motion of the tide-wave in a more compendious manner than by obtaining independently the exact establishment of many different but adjacent places. We may compare the times of high water at such places immediately with one another, and thus trace the motion of the tide-wave directly.

If, in travelling along any coast, we observe every day the time of high water, we can determine whether the establishment becomes later or earlier as we advance; for the effect of the half-monthly establishment is known, at any rate approximately, if we know the age of the tide exactly; and by correcting for this we shall see how the establishments of different places are related to each other, and thus in what direction the tide-wave is moving.

This mode of observation would, however, be open to considerable inaccuracy, since accidental causes often accelerate or retard the time of high water by a quarter of an hour, or in some cases much more. This inaccuracy may in a great measure be remedied *by comparing each day's observation with those at a neighbouring place, where constant observations are made.* By this means the course of the tide-wave might be traced with considerable correctness by means of a single observation at each place.

This method of comparing the observed time of high water at any place with the time at some standard place in the neighbourhood, would be attended with great advantages. It resembles in principle the method adopted by astronomers of comparing the places of small stars and other celestial objects with that of some principal star in their neighbourhood.

By such observations as we have just spoken of, it would be easy to determine the points of divergence and the points of convergence of cotidal lines; that is, the places where the tide hour is earlier than it is at places adjacent on each side, and the places at which it is later. The determination of such points would be highly important for the purpose of enabling us to trace the course of the cotidal lines, and ought to be attended to in every maritime survey.

In a coast broken into bays, there would be many points of divergence and convergence on a small scale. In order to determine the general course of the tide-wave, it would be proper to make observations at points similarly situated; for instance, at all the principal promontories, or in the parts of the bays nearest the open sea.

It would also be proper to attend to the course of the flood and ebb streams, so as to determine from what quarter the tide comes according to the common notions on the subject. It is to be recollected, that this is not necessarily the quarter from which the tide-wave comes; but a knowledge of one fact may be of service in determining the other. In channels where the stream of flood and ebb run alternately in opposite directions, it is highly important that the time of *slack water*, as well as of high and low water, should be noted.

The publication of tide observations, as originally made, may be of the same use as the same proceeding in other departments of astronomy. It enables all, who are able and willing, to employ such observations in exemplifying, correcting, and extending the theory, and in verifying what is done by others in these ways. The publication of the Sheerness Tide Observations, recently directed by the Council of the Society, may thus be a useful step. It may be noticed that the Sheerness tides, like the rest of the Thames tides, are probably compound, being derived partly from the north and partly from the south branch of the British tide. On this account, some of the circumstances may be disguised in those tides and in the London ones, which would appear in simple tides; for instance, the difference of the two semidiurnal tides. The tides of Plymouth, Falmouth, Penzance, or some point in the Scilly Isles, would in this respect be more instructive, and would offer an interesting comparison with those of Brest.

Conclusion.

I cannot conclude this memoir without again expressing my entire conviction of its very imperfect character.—I should regret its publication if I supposed it likely that any intelligent person could consider it otherwise than as an attempt to combine such information as we have, and to point out the want and the use of more. I shall be neither surprised nor mortified if the lines which I have drawn shall turn out to be in many instances widely erroneous: I offer them only as the simplest mode which I can now discover of grouping the facts which we possess. The lines which occupy the Atlantic, and those which are near the coasts of Europe, appear to have the greatest degree of probability. The tides on the coasts of New Zealand and New Holland have also a consistency which makes them very probable. The Indian Ocean is less certain; though it is not easy to see how the course of the lines can be very widely different from that which we have taken. The course of the lines in the Pacific appears to be altogether problematical; and though those which are drawn in the neighbourhood of the west coast of America connect most of the best observations, they can hardly be considered as more than conjecture:—in the middle of the Pacific I have not even ventured to conjecture.

It only remains to add, that I shall be most glad to profit by every opportunity of improving this Map, and will endeavour to employ for this purpose any information with which I may be supplied.

Good tide-observations, at almost any place, will be valuable for this purpose, if the local circumstances be known. The following may be mentioned as instances in which such observations would be of more peculiar value.

1. *Good observations* at the Cape of Good Hope, at Van Diemen's Land, at Swan River, and at some place on the east coast of New Zealand: also at any place on the south or east coast of Ceylon; at the Mauritius, and generally at any of the islands in the Indian Ocean. Such observations would serve to decide the general position of the cotidal lines.

We want also observations on the west coast of America and at some of the islands of the Pacific, where the tides are sufficiently large to be clearly distinguishable from a rise or fall of the surface of the sea due to other causes: but such observations must be made at several places before they can be connected in any very probable manner.

2. *Good and long-continued observations* at any of the above places, or at other places, whether in an open part of the ocean, or not: for instance, those made within the inbend of deep sounds and bays, or on the banks of rivers, would be no less valuable than those on an open coast, or detached island, for certain purposes. These observations would enable us to determine the

age of the tide, the difference of the two semidiurnal tides, the effect of the parallax and declination of the moon, and many other circumstances of great interest.

3. *Comparative simultaneous tide-observations* at different places on the same line of coast. These would enable us better than any other observations to determine the motion of the tide-wave along the coast.

Probably tide-observations at all the points of the coast of England where the officers of the Preventive Service are stationed, carefully made and continued for a fortnight, would give us a clearer view of the progress of the tide along our coasts than we can obtain by any means at present extant.

In surveying any coast, one part of the proceeding ought to be to station an observer at one point to observe the tides constantly, and to employ others to move from place to place, noting the tides at each, so that they may afterwards be compared with those at the tide-station.

⁂ This Paper is accompanied by two Charts;—namely, A general one, representing the greater part of the world, and A Chart of the British Isles; on which THE COTIDAL LINES are drawn, according to the observations adduced and discussed in the preceding Memoir.

METEOROLOGICAL JOURNAL,

KEPT BY THE ASSISTANT SECRETARY

AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF

THE PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL FOR JULY, 1832.

1832. July.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
						Fahrenheit.		Self-registering.				
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		9 A.M.	3 P.M.	Lowest.	Highest.			
☉ 1	30.549	74.9	30.267	73.3	56	68.8	74.9	54.6	76.3		WSW	A.M. Lightly overcast. P.M. Fine and cloudless—light wind.
☽ 2	30.236	70.6	30.200	73.2	55	66.3	71.0	58.3	73.7		N	Fine and cloudless.
♂ 3	30.176	71.9	30.109	71.2	61	61.8	69.7	54.3	70.0		ENE	A.M. Lightly overcast. P.M. Fine and cloudless.
☿ 4	30.038	68.8	29.984	71.7	59	63.2	73.8	54.2	75.3		SE	Fine—light clouds.
♃ 5	30.048	75.4	30.005	74.3	59	69.2	76.5	58.7	77.4		NNW	Fine—cloudy. Evening lowering—light rain.
♀ 6	29.955	75.3	29.867	74.8	56	69.7	72.5	59.8	74.8		SSW	Fine—lightly overcast.
♄ 7	29.760	71.5	29.867	72.9	58	65.0	70.9	54.7	73.2		WSW	Fine—cloudy.
☉ 8	29.935	71.7	29.893	72.3	63	65.4	67.3	60.4	70.7		WSW	Cloudy. Brisk wind P.M.
☽ 9	29.980	75.6	30.018	74.0	57	67.9	73.2	60.8	75.6		S	Fine—cloudy. Light brisk wind P.M.
♂ 10	29.989	71.7	29.947	73.2	62	67.6	70.8	61.3	71.7		WSW	Cloudy. Fine P.M.
☿ 11	29.805	71.3	29.875	73.0	56	67.2	73.6	61.3	74.4		W	Fine and clear—light clouds.
○ ♃ 12	29.921	71.7	29.869	74.0	57	66.7	72.3	59.4	77.8		SSW	Fine—slight clouds. Thunder and hail with heavy rain at 6½h. P.M.
♀ 13	29.843	72.6	29.920	74.7	64	70.3	72.8	62.3	76.4	0.417	WSW	Fine and clear. Evening lowering.
♄ 14	30.023	68.8	30.051	72.0	61	62.8	70.4	60.9	71.0	0.500	N	A.M. Heavy rain at 8½h. P.M. Fine—cloudy.
☉ 15	30.443	76.5	30.418	73.6	53	65.3	71.0	53.6	72.4	0.058	N	Clear and cloudless. Light clouds and wind, P.M.
☽ 16	30.325	69.4	30.239	72.3	61	64.5	73.6	58.3	74.4		W	A.M. Cloudy. P.M. Fine—nearly cloudless.
♂ 17	30.228	77.4	30.122	74.9	57	72.6	80.4	59.7	81.2		NW	Fine and cloudless. Evening lowering.
☿ 18	30.095	72.6	30.119	72.8	48	62.7	67.7	58.7	69.3		NNW	Fine and clear—light clouds and wind.
♃ 19	30.191	72.3	30.196	69.9	47	59.9	63.8	49.6	65.3		N	Fine and clear—light clouds.
♀ 20	30.238	69.4	30.191	70.3	46	60.0	64.4	51.4	66.3		N	Fine. A.M. Cloudless. P.M. Light clouds.
♄ 21	30.192	70.4	30.176	68.2	48	59.7	59.8	50.4	63.3		N	A.M. Fine and cloudless. P.M. Lightly overcast.
☉ 22	30.227	66.9	30.229	65.8	51	60.0	60.0	52.3	61.4		N	Clear—cloudy.
☽ 23	30.227	66.7	30.190	66.8	50	60.6	64.0	50.3	65.3		NNW	Fine—lightly overcast.
♂ 24	30.224	68.8	30.233	69.0	56	63.2	66.3	53.8	69.4		N	Fine and clear—light clouds.
☿ 25	30.239	66.7	30.192	69.3	63	63.2	70.2	57.6	71.5		W	Lightly cloudy.
♃ 26	30.192	67.7	30.162	70.2	55	64.4	69.0	56.3	70.0		N	Fine—lightly cloudy.
● ♀ 27	30.165	69.3	30.141	69.1	59	63.3	65.2	53.6	66.5		N	A.M. Cloudy. P.M. Fine—light clouds.
♄ 28	30.246	69.3	30.231	68.8	53	62.2	68.5	49.8	69.4		NNE	Clear and cloudless.
☉ 29	30.359	63.3	30.367	68.3	57	61.3	68.2	54.2	68.7		N	Clear—cloudy.
☽ 30	30.407	68.1	30.370	70.2	61	61.4	68.5	53.4	70.0		E	Fine. A.M. Haze. P.M. Light clouds.
♂ 31	30.334	63.3	30.257	67.3	55	56.2	64.3	55.3	65.7		E	Overcast.
	Mean 30.146	Mean 70.6	Mean 30.120	Mean 71.3	Mean 56.3	Mean 64.3	Mean 69.5	Mean 56.1	Mean 71.2	Sum 0.975		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. …………	9 A.M.	3 P.M.
	30.036	30.006

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge ………………… =83 feet 2½ in.

……………………………………………… above the mean level of the Sea (presumed about) ………… =95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ………………… =79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR AUGUST, 1832.

1832. August.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
						Fahrenheit.		Self-registering.				
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		9 A.M.	3 P.M.	Lowest.	Highest.			
☿ 1	30.111	67.8	30.039	68.9	57	63.7	64.4	52.0	68.8		E	Fair—lightly overcast.
♃ 2	29.867	64.9	29.829	70.5	56	64.0	73.3	56.2	77.5	0.138	E	A.M. Fine. P.M. Thunder-storm at 5¾ h.
♀ 3	29.932	68.3	29.944	70.8	57	63.4	69.2	61.3	72.3	0.543	SW var.	Lightly cloudy.
♄ 4	30.083	69.8	30.097	71.9	58	64.0	69.6	56.8	72.8		SSW	A.M. Fine. P.M. Rain.
☉ 5	29.982	66.5	29.936	68.9	59	62.7	65.0	60.9	66.3	0.366	SE var.	Rain.
☽ 6	29.917	70.0	29.915	70.6	56	64.2	71.4	55.2	72.7	0.058	SW	Fine and clear—light clouds.
♂ 7	29.948	69.3	29.974	71.5	59	68.2	70.8	59.2	72.6		SW	Fine. A.M. Light clouds. P.M. Nearly cloudless.
☿ 8	30.095	71.9	30.067	72.0	57	67.3	74.6	55.4	74.8		S	Fine. A.M. Light haze. P.M. Clear.
♃ 9	30.089	69.5	30.085	73.3	60	69.3	74.9	58.3	76.8		ESE	Clear and cloudless.
♀ 10	30.168	74.0	30.178	75.4	61	72.0	75.0	62.5	81.8		SSW	Fine. A.M. Cloudless. P.M. Light clouds.
○ ♄ 11	30.342	76.0	30.315	76.2	62	71.7	77.7	64.4	79.6		S	Fine. A.M. Cloudless. P.M. Light clouds.
☉ 12	30.328	73.8	30.235	75.5	59	66.6	75.8	57.5	77.2		WSW	Fine—light clouds and haze.
☽ 13	29.978	69.4	29.922	72.7	59	62.8	70.1	61.9	70.8		N	Lightly overcast.
♂ 14	29.956	70.6	29.911	72.4	58	64.9	71.7	57.8	72.2		NE	Fine—light haze and clouds.
☿ 15	29.909	69.3	29.883	73.3	62	64.2	73.8	61.4	74.7		SSE	Fine—lightly cloudy.
♃ 16	29.982	72.4	30.006	73.7	61	68.5	75.8	59.2	76.8		SW	Fine—light clouds and wind.
♀ 17	30.143	71.8	30.117	72.4	57	65.4	72.2	54.8	73.8		SSW	Fine. A.M. Cloudless. P.M. Light clouds.
♄ 18	29.944	69.9	29.873	71.0	60	66.4	65.8	57.8	69.5		S	Lightly overcast. Light rain.
☉ 19	29.758	68.0	29.796	72.4	58	63.3	70.5	60.0	72.0		SE var.	A.M. Light rain. P.M. Fine—light clouds.
☽ 20	30.070	68.8	30.007	71.2	58	63.8	71.0	53.4	73.0	0.041	SSW	A.M. Fine and clear. P.M. Cloudy.
♂ 21	29.922	68.8	29.863	70.3	59	66.2	68.0	62.4	69.7		WSW	A.M. Fine. P.M. Overcast.
☿ 22	29.732	68.6	29.780	71.6	59	63.8	64.0	58.8	72.2	0.152	S	Cloudy—light rain P.M.
♃ 23	29.934	70.3	29.905	71.4	58	64.7	68.3	54.8	70.8	0.094	SW	A.M. Fine. P.M. Cloudy.
♀ 24	30.069	67.3	30.057	69.5	53	60.4	68.2	51.0	70.8		SSW	Fine—light clouds and haze.
● ♄ 25	29.845	67.8	29.784	68.9	59	61.4	65.6	56.2	66.6		S var.	A.M. Heavy rain. P.M. Fine—light clouds.
☉ 26	29.701	66.3	29.726	68.4	54	59.2	68.8	52.5	65.7	0.291	SSW	Fine—lightly overcast.
☽ 27	29.742	64.6	29.617	65.8	52	60.2	60.3	47.0	64.2		S var.	Cloudy. A.M. Brisk wind. P.M. Light rain.
♂ 28	29.211	64.8	29.158	66.2	55	60.4	63.0	56.5	66.2		S	Overcast. P.M. Light rain.
☿ 29	29.328	60.4	29.322	61.7	53	54.5	56.5	51.8	58.2	0.061	WSW	Overcast—light rain.
♃ 30	29.512	61.7	29.595	64.2	54	58.2	59.8	54.2	62.0	0.186	S	A.M. Lightly cloudy. P.M. Light rain.
♀ 31	29.760	62.7	29.734	64.9	52	57.8	64.0	49.0	67.5	0.102	SW	Fine—light clouds and haze.
	Mean 29.915	Mean 68.6	Mean 29.893	Mean 70.6	Mean 57.5	Mean 64.0	Mean 69.0	Mean 56.8	Mean 71.3	Sum 2.032		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. .... { 9 A.M. 29.809 — 3 P.M. 29.781 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge .... =83 feet 2¼ in.

.... above the mean level of the Sea (presumed about)............... =95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House .... =79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR SEPTEMBER, 1832.

1832. Sept.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		Fahrenheit.		Self-registering.				
						9 A.M.	3 P.M.	Lowest.	Highest.			
♄ 1	29.643	64.2	29.653	66.4	54	60.8	66.3	55.5	69.0	0.172	WSW	Fine—light clouds.
☉ 2	29.948	62.8	30.029	65.0	53	59.0	64.4	50.5	66.2	0.038	SW	Fine—light clouds.
☽ 3	30.213	62.6	30.196	65.6	53	58.8	66.5	51.3	67.8		W	Fine—lightly cloudy.
♂ 4	30.300	62.6	30.225	65.2	55	56.7	65.0	51.7	65.7		NNE	Fine and cloudless—hazy.
☿ 5	30.112	62.4	30.055	65.2	54	57.8	64.0	51.6	65.7		NNE	Fine and clear—light clouds.
♃ 6	30.004	61.7	29.924	64.8	55	60.0	63.5	52.6	65.2		ESE	Fine—light clouds and wind. At 9 P.M. thunder and lightning, with light rain.
♀ 7	29.850	63.6	29.871	64.8	60	61.1	64.2	55.4	64.7		NNE	Overcast.
♄ 8	30.051	62.3	30.035	66.0	59	59.4	68.0	54.6	68.3		W	Fine. A.M. Cloudless. P.M. Light clouds.
☉ 9	30.025	62.7	29.962	66.7	59	59.3	65.9	53.3	68.3		WSW	Fine—lightly cloudy.
○ ☽ 10	29.781	63.5	29.808	66.2	59	59.3	64.8	56.3	66.4	0.014	SSW	A.M. Rain. P.M. Fine—light clouds.
♂ 11	30.108	62.7	30.171	65.2	51	58.3	62.8	50.4	64.2	0.083	WNW	Fine—lightly cloudy and overcast.
☿ 12	30.337	61.4	30.279	64.7	51	55.9	62.8	47.3	64.3		WSW	A.M. Fine and clear. P.M. Cloudy.
♃ 13	30.074	61.3	29.986	63.6	60	59.7	61.5	55.6	66.7		SSW	A.M. Overcast—deposition. P.M. Fine and clear.
♀ 14	29.908	60.3	29.863	63.2	46	54.8	60.5	48.6	62.3		NW var.	Fine—light wind. A.M. Cloudless. P.M. Light clouds.
♄ 15	29.950	60.3	30.045	62.8	52	56.3	60.0	52.2	61.7		NNW	Fine—light clouds.
☉ 16	30.242	58.6	30.265	62.7	54	56.6	62.3	48.7	63.4		WNW	Lightly cloudy.
☽ 17	30.301	60.3	30.224	63.6	54	59.2	64.3	55.3	65.6		SW	A.M. Overcast. P.M. Nearly cloudless.
♂ 18	29.992	61.3	30.016	63.0	55	59.8	63.0	55.4	65.6		SSW	A.M. Overcast. P.M. Fine—light shower.
☿ 19	30.336	57.3	30.376	59.3	42	51.3	56.9	42.7	56.9		NNE	Clear and cloudless—light wind.
♃ 20	30.541	55.5	30.515	60.0	47	49.8	61.2	41.6	61.4		NNW	Fine and cloudless.
♀ 21	30.557	56.7	30.519	62.0	51	52.4	62.8	46.4	63.4		NNW	Fine. A.M. Cloudless. P.M. Light clouds.
♄ 22	30.526	59.6	30.453	63.0	57	58.0	61.6	52.6	62.3		E	Overcast.
☉ 23	30.377	59.0	30.322	62.7	53	56.3	67.3	49.6	68.7		E	Clear and cloudless.
● ☽ 24	30.399	59.4	30.366	63.8	53	56.2	71.0	50.4	71.7		ESE	Fine and cloudless. Hazy A.M.
♂ 25	30.442	62.5	30.384	66.0	58	58.8	71.9	53.9	72.5		SSW	Cloudless—hazy.
☿ 26	30.326	63.3	30.230	66.9	56	58.7	69.0	51.7	70.7		SW	Fine and cloudless—hazy.
♃ 27	30.192	63.9	30.126	67.7	58	58.9	69.7	53.8	70.7		SW	Clear and cloudless.
♀ 28	30.083	62.6	30.045	66.4	55	56.3	68.6	50.3	69.4		SW	Fine. A.M. Cloudless. P.M. Light clouds.
♄ 29	30.040	63.3	29.952	67.5	56	58.6	72.9	54.6	72.9		SW	A.M. Strong haze. P.M. Fine—light clouds.
☉ 30	29.950	66.3	29.961	67.9	62	63.6	63.6	58.7	65.6	0.047	S	A.M. Heavy showers. P.M. Fine—cloudy.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	30.154	6.15	30.129	64.6	54.4	57.7	64.9	51.8	66.2	0.354		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A.M.	3 P.M.
	30.070	30.035

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge .... = 83 feet 2¼ in.

.... above the mean level of the Sea (presumed about) .... = 95 feet.

The external Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House .... = 79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR OCTOBER, 1832.

1832. October.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		Fahrenheit.		Self-registering.				
						9 A.M.	3 P.M.	Lowest	Highest.			
☽ 1	30.063	64.6	30.018	67.3	59	59.8	65.0	55.6	66.7	0.250	SSW	Fine—lightly cloudy.
♂ 2	29.938	64.3	29.940	67.5	61	61.8	65.2	58.3	66.3	0.228	SSW	Fine—lightly cloudy.
☿ 3	29.934	63.3	29.849	66.0	58	58.7	63.5	52.7	64.5		S	Fine—lightly cloudy. Clear A.M.
♃ 4	29.781	64.2	29.703	65.3	60	62.0	62.9	58.8	63.6		SSW	Cloudy—showery. High wind, with rain, at night.
♀ 5	29.399	62.6	29.144	63.8	58	58.8	59.2	54.7	60.4	0.258	SSE	Overcast—heavy showers.
♄ 6	29.528	59.7	29.536	62.5	49	53.8	55.2	48.7	59.7	0.139	S	A.M. Fine and clear. P.M. Heavy showers.
☉ 7	29.582	58.2	29.659	61.3	52	53.6	58.6	45.8	60.7	0.306	SW	Fine and clear. Showers, morning and evening. At night, high wind, with rain.
☽ 8	29.195	58.8	29.429	59.5	53	53.3	54.6	53.3	55.6	0.278	WSW	Cloudy—light unsteady wind.
○ ♂ 9	29.878	55.3	29.977	60.7	47	51.1	57.2	44.3	59.3		W	Clear and cloudless. Light rain at night.
☿ 10	30.029	58.9	30.122	62.4	59	59.7	63.4	50.7	64.3		WSW	A.M. Foggy. P.M. Fine and clear—light clouds.
♃ 11	30.313	62.3	30.321	65.6	60	61.7	66.4	57.7	68.2		SW	Fine—lightly overcast.
♀ 12	30.106	63.7	29.976	65.8	59	60.7	61.7	58.6	63.3		SSW	Cloudy.—Rain at night.
♄ 13	29.938	60.7	29.982	63.3	48	53.1	58.6	50.2	59.4	0.133	WSW	Fine. A.M. Cloudless. P.M. Light clouds.
☉ 14	30.335	57.7	30.306	61.3	47	47.9	57.2	43.7	57.5		WSW	Fine—light clouds and haze.
☽ 15	30.264	57.7	30.210	61.4	52	52.0	59.0	47.5	60.3		SW	Fine and clear—light clouds. Rain at night.
♂ 16	30.195	58.9	30.228	61.6	55	55.9	57.9	52.3	59.3		SW	A.M. Light rain. P.M. Fine and clear.
☿ 17	30.386	56.3	30.361	60.2	47	47.9	57.2	44.2	57.2	0.022	N	A.M. Lightly overcast. P.M. Fine—light clouds.
♃ 18	30.306	57.2	30.261	59.3	53	53.0	56.2	48.0	56.2		WSW	Overcast—hazy.
♀ 19	30.255	57.5	30.243	58.7	52	52.2	53.9	51.5	53.9	0.061	N	A.M. Rain and fog. P.M. Overcast.
♄ 20	30.338	52.9	30.332	56.3	40	42.9	52.8	39.4	52.8	0.069	NNW	Fine and cloudless—light haze.
☉ 21	30.377	55.4	30.307	57.3	49	49.3	54.6	42.5	54.6		N	A.M. Foggy. P.M. Fine and clear—light clouds.
☽ 22	30.261	51.8	30.249	55.8	46	46.0	53.8	42.3	54.6		ENE	Clear and cloudless.
● ♂ 23	30.354	52.3	30.332	56.3	49	49.5	53.9	43.3	54.7		NE	A.M. Hazy. P.M. Fine.
☿ 24	30.364	53.3	30.375	54.8	49	49.3	50.1	45.9	51.3		NE	Overcast—hazy.
♃ 25	30.455	51.7	30.392	53.7	46	46.9	52.0	45.0	52.0		E	A.M. Fog. P.M. Fine—light haze.
♀ 26	30.397	51.9	30.356	52.6	46	47.8	50.9	45.4	50.9		NE	Fog.
♄ 27	30.380	50.4	30.342	52.2	44	44.3	50.9	42.5	50.9		NE	Fog.
☉ 28	30.258	51.9	30.182	54.9	49	49.5	53.4	43.8	53.4	0.097	E	Fog. Deposition A.M.
☽ 29	29.984	53.4	29.909	54.9	52	52.8	55.2	49.3	55.7	0.017	S	Overcast—light fog—deposition.
♂ 30	30.182	51.5	30.077	55.0	43	44.8	52.4	41.3	52.4	0.153	SSW	Fine and cloudless—light haze.
☿ 31	30.119	52.3	30.088	54.7	44	47.2	52.9	44.3	56.3	0.014	WSW	A.M. Cloudless. P.M. Cloudy. At night, rain with wind.
	Mean 30.093	Mean 57.1	Mean 30.071	Mean 59.7	Mean 51.2	Mean 52.5	Mean 57.0	Mean 48.4	Mean 57.9	Sum 2.025		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. …………	9 A.M.	3 P.M.
	30.022	29.992

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge …………………… =83 feet 2 ½in.

…………………………………… above the mean level of the Sea (presumed about) ………… =95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ……………………… =79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR NOVEMBER, 1832.

1832. Nov.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		Fahrenheit.		Self-registering.				
						9 A.M.	3 P.M.	Lowest.	Highest.			
♃ 1	29.590	55.4	29.748	57.7	56	56.6	57.2	46.7	59.3	0.161	WNW	A.M. Rain, early. P.M. Cloudless—light haze.
♀ 2	29.814	55.0	29.627	56.7	51	51.5	57.7	48.6	57.7		SSE	Overcast—light haze. At midnight, brisk wind, with heavy rain.
♄ 3	29.715	56.7	29.826	58.2	49	52.7	53.5	51.3	54.5	0.036	NNW	Fine—light clouds and haze.
☉ 4	29.867	54.4	29.776	55.7	43	47.3	49.4	45.8	49.4		NW	Fair—light clouds. Evening, rain.
☽ 5	29.703	47.8	29.823	49.4	38	39.3	42.9	36.7	42.9	0.050	NNW	A.M. Overcast—deposition. P.M. Fine—light clouds.
♂ 6	30.247	47.3	30.292	49.0	41	42.9	46.0	38.7	47.2		NNW	Fine—light brisk wind.
☿ 7	30.413	46.0	30.338	48.2	39	41.9	45.9	39.3	45.9		N	A.M. Cloudy—light fog. P.M. Fine and cloudless.
○ ♃ 8	30.034	45.2	29.922	45.9	38	43.3	42.0	38.7	42.3		N	Overcast—light fog and wind. Light rain P.M.
♀ 9	29.899	44.0	29.877	44.5	36	39.3	40.6	37.3	43.7		N	Fog.
♄ 10	29.713	44.4	29.544	46.8	43	45.1	47.8	36.7	51.7		ESE	Fog—light wind. Rain P.M.
☉ 11	29.596	47.2	29.590	49.6	44	44.1	48.2	41.3	49.5		W	A.M. Fog. P.M. Fine and clear. Evening, rain.
☽ 12	29.706	47.4	29.715	47.7	42	42.8	44.3	42.2	44.3	0.200	WSW	A.M. Cloudless—haze. P.M. Heavy fog.
♂ 13	29.790	46.3	29.770	47.2	39	39.0	42.9	38.4	48.3		E	A.M. Heavy fog. P.M. Cloudy.
☿ 14	29.723	48.7	29.726	51.3	47	48.9	51.0	38.3	51.0		S	Overcast—light fog. Light rain P.M.
♃ 15	29.889	51.3	29.960	52.0	48	48.2	48.5	47.3	48.7		ESE	Overcast—fog.
♀ 16	30.275	49.8	30.311	52.0	43	43.3	49.6	42.5	49.6		NNE	Fine—light clouds and wind.
♄ 17	30.411	48.3	30.348	50.0	42	42.5	45.2	39.8	46.4		N	Fine—light clouds and haze.
☉ 18	30.193	48.3	30.095	48.7	42	42.3	42.3	40.3	43.3		E	Overcast—light haze.
☽ 19	29.940	47.5	29.863	48.7	44	44.3	46.0	41.7	46.2		E	Lightly overcast—haze.
♂ 20	29.738	49.4	29.669	51.5	45	45.8	49.3	43.8	49.3		E	Fine—lightly overcast.
☿ 21	29.594	49.7	29.536	51.2	45	45.3	47.6	43.8	47.7		E	A.M. Fog. P.M. Fine and cloudless.
● ♃ 22	29.784	50.4	29.837	51.9	44	46.5	49.0	44.7	49.0		ESE	Fine and cloudless—light haze.
♀ 23	29.954	49.3	29.948	52.3	44	45.8	53.3	41.3	53.3		ENE	Fine and cloudless.
♄ 24	30.052	52.4	30.014	54.7	51	51.1	54.0	44.7	54.0		E	A.M. Fog—deposition. P.M. Fine—light clouds.
☉ 25	29.891	53.8	29.869	54.4	51	50.8	49.2	49.7	50.8	0.131	ESE	A.M. Fog and rain. P.M. Fine and clear.
☽ 26	29.564	51.7	29.586	52.8	45	45.4	49.2	43.6	49.2	0.228	WSW	A.M. Cloudless. P.M. Fair—light clouds.
♂ 27	29.736	49.2	29.564	50.7	41	41.3	45.4	37.9	45.9		WSW	Overcast. A.M. Light fog. P.M. Light rain.
☿ 28	29.654	48.7	29.762	50.2	39	40.7	48.2	39.3	48.3		WSW	Fine and Cloudless.
♃ 29	29.614	50.3	29.604	50.9	43	43.4	45.2	40.2	45.7	0.264	WSW	Fine and Cloudless. Rain, with high wind, early A.M.
♀ 30	29.855	46.4	29.821	48.6	38	40.0	44.7	36.3	53.8		SW	A.M. Fair—light clouds. P.M. Light rain.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.865	49.4	29.845	51.0	43.7	45.0	47.9	41.9	49.0	1.070		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ………… { 9 A.M. 29.817 — 3 P.M. 29.792 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge……………………… = 83 feet 2¼ in.

……………………………………… above the mean level of the Sea (presumed about) ………… = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ……………………… = 79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR DECEMBER, 1832.

1832. Dec.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
						Fahrenheit.		Self-registering.				
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		9 A.M.	3 P.M.	Lowest.	Highest.			
♄ 1	29.819	51.7	29.843	53.9	51	54.5	55.8	39.3	55.8	0.063	W	Overcast—light fog.
☉ 2	29.679	55.5	29.622	56.3	53	54.0	52.2	54.0	54.6		WSW	Fine—light clouds. A.M. Rain, early. P.M. Hailstorm, with thunder, at 6½h.
☽ 3	29.499	50.7	29.520	51.3	43	44.8	45.3	42.2	46.4	0.111	W	A.M. Cloudless. P.M. Light rain.
♂ 4	29.944	48.6	30.025	49.9	40	43.6	46.6	41.3	46.6	0.028	NNW	A.M. Cloudless. P.M. Fine—light clouds.
☿ 5	30.218	46.3	30.172	47.7	40	40.3	42.5	37.8	43.8		N	A.M. Overcast. P.M. Fine—light clouds.
♃ 6	30.018	47.6	30.030	49.3	44	44.9	46.4	39.6	46.4	0.117	NNE	Overcast—foggy—deposition.
○ ♀ 7	30.280	47.8	30.315	48.3	42	42.1	42.4	40.3	43.8		N	Strong haze.
♄ 8	30.366	47.2	30.358	48.8	42	42.0	45.9	37.7	46.7		SW	Overcast—fog.
☉ 9	30.403	50.2	30.380	51.3	47	47.8	48.2	41.4	48.2		WSW	Overcast—fog. Deposition, A.M.
☽ 10	30.366	50.3	30.328	51.2	45	46.8	47.9	45.6	48.2		S	A.M. Fine—light haze. P.M. Light rain.
♂ 11	30.433	48.6	30.433	50.9	41	41.3	48.2	38.7	48.2		WSW	Overcast—light fog.
☿ 12	30.479	50.3	30.439	51.3	43	44.8	47.8	40.7	47.8		W	Overcast—light fog.
♃ 13	30.253	59.8	30.119	51.2	44	44.8	45.8	42.7	45.8		SSW	Lightly overcast.
♀ 14	29.950	49.8	29.962	49.9	47	47.1	44.0	39.0	51.3		WSW	Strong haze—deposition.
♄ 15	29.574	51.0	29.688	50.0	51	51.8	44.2	40.3	51.8	-	WSW	Light unsteady wind. A.M. Rain. P.M. Overcast.
☉ 16	30.014	45.6	29.950	47.6	33	36.0	42.2	33.9	52.3	0.194	W	Lightly overcast. Rain, with high wind, at night.
☽ 17	29.552	49.3	29.483	51.4	52	52.7	52.3	35.3	54.8	0.014	WSW	A.M. Overcast—deposition—light fog. P.M. Fine.
♂ 18	29.518	48.7	29.610	49.4	43	43.4	44.2	42.3	44.2		WSW	A.M. Strong high wind, till 4 o'clock. P.M. Fine and cloudless.
☿ 19	29.712	45.4	29.695	46.9	37	37.2	41.5	34.8	41.5		WSW	A.M. Overcast. P.M. Fine.
♃ 20	29.908	43.1	29.994	43.5	34	34.7	37.9	33.3	41.7		NNW	Hazy—light wind. Cloudless A.M.
♀ 21	29.676	44.3	29.667	46.2	43	43.0	46.4	33.9	46.5	0.022	S	A.M. Fog—deposition. P.M. Fine.
● ♄ 22	29.819	47.2	29.843	48.9	46	46.2	48.9	34.0	49.7		WSW	Fine and cloudless. Fog and deposition, A.M.
☉ 23	29.669	50.7	29.709	52.4	49	50.4	50.4	44.7	52.6		SSE	Fair—lightly cloudy.
☽ 24	30.032	49.8	29.998	50.4	44	44.8	45.0	43.8	52.3		S	A.M. Overcast—light fog. P.M. Fine and cloudless.
♂ 25	29.645	52.6	29.804	53.4	52	52.8	49.2	44.3	53.4	0.061	S	Cloudy—light brisk wind. Strong wind, early A.M.
☿ 26	30.135	46.7	30.161	48.4	38	38.2	43.8	36.3	43.8	0.019	WSW	Cloudless—light haze.
♃ 27	30.191	45.8	30.162	46.7	39	39.2	41.6	36.7	41.6		WSW	A.M. Overcast—fog. P.M. Fine and cloudless.
♀ 28	30.088	42.8	30.035	43.3	34	34.0	39.9	32.3	39.9		S	A.M. Strong fog. P.M. Fine—light clouds and haze.
♄ 29	29.715	44.4	29.635	43.8	40	40.1	40.0	33.3	40.1		ESE	Overcast—light haze and wind. Rain P.M.
☉ 30	30.073	42.3	30.119	42.8	36	36.9	38.7	35.8	38.7	0.111	N	Lightly cloudy—haze.
☽ 31	29.812	41.3	29.960	42.2	36	36.3	39.2	34.3	39.3		NW	A.M. Fog. Snow, early. P.M. Light rain.
	Mean 29.963	Mean 48.2	Mean 29·970	Mean 49.0	Mean 42.9	Mean 43.8	Mean 45.3	Mean 39.0	Mean 47.0	Sum 0.740		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ……………	9 A.M.	3 P.M.
	29.918	29.923

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